# MEMOIRS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

BRADSHAW AND BLACKLOCK, PRINTERS, MANCHESTER AND LONDON.

# MEMOIRS

OF THE

# LITERARY

AND

# PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

Second Series.

VOLUME NINTH.

LONDON:
H. BAILLIERE, PUBLISHER, 219, REGENT STREET,
AND 290, BROADWAY, NEW YORK.
PARIS: J. B. BAILLIERE, LIBRAIRE, RUE HAUTEFEUILLE.
BRADSHAW & BLACKLOCK, 47, BROWN STREET,
MANCHESTER.

MDCCCLI.

## NOTE.

The Authors of the several Papers contained in this Volume, are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

# CONTENTS.


ARTICLE. PAGE

I.—Report of PETER CLARE, F.R.A.S., and JOHN FREDERIC BATEMAN, F.G.S., M. Inst. C.E., being the Committee appointed for superintending the Measurement of Rain falling along the Lines of the Rochdale, Ashton-under-Lyne, and Peak-Forest Canals.
With Observations upon the Returns, and other particulars. By JOHN FREDERIC BATEMAN ........ 1

II.—On Mnemonic Aids in the Study of Analysis. By REV. THOMAS P. KIRKMAN, M.A., Rector of Croft-with-Southworth, Lancashire ........ 29

III.—On the Formation of Dew. By THOMAS HOPKINS ...... 46

IV.—On Lightning and Lightning Conductors. By WILLIAM STURGEON ........ 56

V.—Researches into the Identity of the Existences or Forces of Light, Heat, Electricity, Magnetism, and Gravitation. By JOHN GOODMAN, M.D. ........ 80

VI.—On Faults in Farming. By JOHN JUST. ........ 93

VII.—Some Remarks on Heat, and the Constitution of Elastic Fluids. By J. P. JOULE, F.R.S., &c. ........ 107

VIII.—Description of a Mineral Vein in the Lancashire Coal Field near Skelmersdale. By E. W. BINNEY.......... 115

IX.—Remarks on a Vein of Lead found in the Carboniferous Strata in Derbyshire, near Whaley Bridge. By E. W. BINNEY ........ 125

X.—Memoir on the Oxides and Nitrates of Lead. By F. C. CALVERT, Professor of Chemistry at Manchester 130

XI.—Description of a Meteorite which fell at Allport in Derbyshire. By ROBERT ANGUS SMITH, PH. D.......... 146

ARTICLE. PAGE

XII.—An Experimental Enquiry into the Relative Powers of the Locomotive Engine, and the Resistance of Railway Gradients. By WM. FAIRBAIRN, F.R.S., V.P. ........ 149

XIII.—On the Security and Limit of Strength of Tubular Girder Bridges constructed of Wrought Iron. By WILLIAM FAIRBAIRN, F.R.S., V.P. ........ 179

XIV.—On the Cause of Unequal Falls of Rain in Cumberland. By ALDERMAN THOMAS HOPKINS ........ 196

XV.—On Impossible and certain other Surd Equations. By ROBERT HARLEY ........ 207

XVI.—On Impossible Equations. By PROFESSOR FINLAY ...... 236

XVII.—On the Composition of the Gas produced by the Joint Distillation of Tar and Water at a high temperature. By JOHN LEIGH, M.R.C.S., F.C.S., &c. ........ 243

XVIII.—On the Chemical Changes attending the Formation of Coal, and on the relation of these Changes to the Philosophy of Gas-making. By JOHN LEIGH, M.R.C.S., F.C.S. ........ 250

XIX.—On Linear Constructions. By REV. THOMAS PENYNGTON KIRKMAN, A.M., Rector of Croft-with-Southworth 279

XX.—On the Analysis of Gaseous Mixtures. By JOHN LEIGH, M.R.C.S., F.C.S. ........ 297

XXI.—A Description of some supposed Meteorites found in Seams of Coal. By E. W. BINNEY. ........ 306

XXII.—On the Volvox Globator. By WILLIAM CRAWFORD WILLIAMSON, Professor of Natural History, Owens College ........ 321

XXIII.—On the Structure and Affinities of the Plants hitherto known as Sternbergiæ. By W. C. WILLIAMSON, Professor of Natural History, Owens College ........ 340

# ERRATA.

| | | | | |
|---|---|---|---|---|
| Page 28—table, line 2nd, | *for* | 51·46 | *read* | 51·64. |
| „ „ „ line 4th, | „ | 24·82 | „ | 26·82; *for* 64·58 *read* 46·58. |
| „ 236—line 11th, | *for* | third and fourth | *read* | fourth and fifth. |
| „ 239— „ 10th, | „ | $-+mR$ | „ | $-mR$. |
| „ 343— „ 28th, | „ | dénaturés | „ | de nature. |
| „ 355— „ 22nd, | „ | foleaceous | „ | foliaceous. |

## LITERARY AND PHILOSOPHICAL SOCIETY

## MANCHESTER.

I.—*Report of* Peter Clare, *F.R.A.S., and* John Frederic Bateman, *F.G.S., M. Inst. C.E., being the Committee appointed for superintending the Measurement of Rain falling along the Lines of the Rochdale, Ashton-under-Lyne, and Peak Forest Canals.*
*With Observations upon the Returns, and other particulars. By* John Frederic Bateman.

Read April 11, 1848.

Three years have elapsed since the last Report on this subject was presented to the Society. During this period the observations have been regularly continued, and the results are as follow. It is almost unnecessary to remark, that the old rain gauges, called the Canal Company's gauges, are placed on the roofs of dwellings, and the Society's gauges on the ground, the observations being taken with a view of ascertaining the difference in the quantity registered in the two situations.

ALONG THE LINE OF THE ROCHDALE CANAL.

| | Slattocks, near Middleton, 450 feet above sea. | | Moss Lock, near Rochdale, about 500 feet above sea. | | White Holme Reservoir, Blackstone Edge, about 1200 feet above sea. | | Toll Bar, Blackstone Edge, about 1000 feet above sea. | | Black House, near Ripponden. | | Sowerby Bridge, 300 feet above sea. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1845. | Canal Co.'s Gauge. | Society Gauge. | Canal Co.'s Gauge. | Society Gauge. | Canal Co.'s Gauge. | Society Gauge. | Canal Co.'s Gauge. | Society Gauge. | Canal Co.'s Gauge. | Society Gauge. | Canal Co.'s Gauge. | Society Gauge. |
| | in. dec. | | in. dec. | | in. dec. | in. dec. | in. dec. | in. dec. | in. dec. | in. dec. | in. dec. | in. dec. |
| January ...... | 1 47 | ... | 1 80 | ... | 2 05 | 3 8 | 2 09 | 4 0 | 1 80 | 2 5 | 1 26 | 1 9 |
| February ... | 1 27 | ... | 1 05 | ... | 0 90 | 1 2 | 0 80 | 1 3 | 0 51 | 1 1 | 0 00 | 0 5 |
| March......... | 2 10 | ... | 1 60 | ... | 2 09 | 5 0 | 2 30 | 5 1 | 3 00 | 4 9 | 1 91 | 2 5 |
| April ......... | 1 85 | ... | 1 40 | ... | 2 30 | 3 0 | 1 99 | 3 0 | 2 00 | 3 0 | 1 17 | 1 9 |
| May...... ..... | 1 30 | ... | 1 08 | ... | 1 90 | 3 8 | 1 86 | 4 0 | 1 80 | 2 7 | 1 81 | 2 1 |
| June ......... | 2 75 | ... | 2 55 | ... | 3 80 | 5 0 | 3 36 | 5 0 | 3 60 | 3 7 | 2 01 | 2 5 |
| July............ | 3 25 | ... | 2 85 | ... | 3 80 | 4 3 | 3 70 | 4 5 | 3 91 | 4 7 | 3 17 | 4 0 |
| August ...... | 4 75 | ... | 5 00 | ... | 5 00 | 8 3 | 4 60 | 8 2 | 4 80 | 8 0 | 4 93 | 5 5 |
| September... | 3 17 | ... | 2 55 | ... | 3 60 | 4 7 | 3 30 | 4 6 | 3 40 | 6 3 | 2 91 | 2 3 |
| October ...... | 3 85 | ... | 3 45 | ... | 4 36 | 8 9 | 4 20 | 8 8 | 4 20 | 7 0 | 3 02 | 3 5 |
| November ... | 2 20 | ... | 2 15 | ... | 2 90 | 4 3 | 2 12 | 4 2 | 2 70 | 5 1 | 1 81 | 2 2 |
| December ... | 4 25 | ... | 3 95 | ... | 7 15 | ... | 6 70 | 12 0 | 7 70 | 12 8 | 4 03 | 5 4 |
| | 32 21 | ... | 29 43 | ... | 39 85 | 52 3 11 mo. | 37 02 | 64 7 | 39 42 | 61 8 | 28 03 | 34 3 |
| **1846.** | | | | | | | | | | | | |
| January ...... | 3 15 | ... | 4 50 | 4 5 | 5 00 | 5 7 | 4 85 | 6 1 | 4 15 | 5 0 | 2 79 | 3 4 |
| February ... | 0 95 | ... | 0 65 | 1 0 | 1 80 | 2 0 | 1 74 | 2 0 | 2 00 | 4 5 | 0 61 | 1 2 |
| March......... | 1 50 | ... | 1 35 | 2 1 | 2 70 | 4 0 | 2 08 | 3 9 | 2 60 | 3 5 | 1 95 | 2 5 |
| April ......... | 2 88 | ... | 2 40 | 3 8 | 4 18 | 5 4 | 4 55 | 5 1 | 4 20 | 5 2 | 5 19 | 5 4 |
| May............ | 1 00 | ... | 0 80 | 1 1 | 1 00 | 0 9 | 1 15 | 1 0 | 1 20 | 1 8 | 0 00 | 0 0 |
| June ......... | 2 00 | ... | 1 70 | 3 0 | 2 60 | 3 1 | 2 72 | 3 5 | 2 68 | 4 0 | 1 84 | 2 6 |
| July............ | 3 30 | ... | 2 75 | 3 0 | 3 20 | 4 1 | 2 80 | 4 0 | 3 40 | 5 2 | 1 31 | 2 9 |
| August ...... | 3 97 | ... | 2 45 | 3 8 | 5 50 | 3 4 | 5 20 | 3 8 | 4 00 | 2 8 | 1 25 | 2 1 |
| September... | 1 20 | ... | 0 85 | 1 4 | 1 06 | 2 6 | 1 00 | 2 5 | 0 90 | 1 0 | 0 79 | 0 9 |
| October ...... | 3 91 | ... | 2 50 | 4 2 | 5 20 | 5 7 | 4 50 | 5 6 | 5 00 | 5 5 | 3 95 | 4 4 |
| November ... | 1 70 | ... | 1 76 | 2 1 | 3 70 | 4 3 | 3 50 | 4 4 | 3 60 | 3 0 | 1 62 | 2 0 |
| December ... | 0 20 | ... | 1 05 | 1 7 | 1 20 | 2 0 | 1 00 | 2 0 | 1 07 | 1 0 | 0 18 | 0 5 |
| | 25 76 | ... | 22 76 | 31 7 | 37 14 | 43 2 | 35 09 | 43 9 | 34 80 | 42 5 | 21 48 | 27 9 |
| 1847. | | | | | | | | | | | | |
| January ...... | 1 00 | ... | 1 10 | 1 4 | 1 90 | 3 2 | 1 70 | 3 1 | 1 80 | 3 0 | 1 55 | 1 6 |
| February ... | 2 50 | ... | 1 41 | 1 9 | 2 90 | 3 8 | 2 63 | 3 4 | 2 70 | 3 0 | 1 32 | 2 2 |
| March......... | 0 90 | ... | 0 84 | d'm'ged | 1 40 | 2 3 | 1 00 | 2 5 | 1 20 | no ret. | 0 95 | 0 9 |
| April ......... | 1 63 | ... | 1 29 | d'm'ged | 2 40 | 3 8 | 2 30 | 4 0 | 2 20 | 2 5 | 1 25 | 2 2 |
| May............ | 2 30 | ... | 3 25 | 3 5 | 3 60 | 3 4 | 3 72 | 3 5 | 3 90 | 3 5 | 2 87 | 3 5 |
| June ......... | 2 90 | ... | 2 55 | 3 1 | 2 60 | 2 5 | 2 47 | 2 5 | 2 57 | 3 4 | 1 20 | 1 9 |
| July............ | 1 30 | .. | 0 80 | 1 6 | 0 65 | 1 1 | 0 75 | 1 3 | 0 60 | 1 3 | 0 25 | 0 8 |
| August ...... | 1 65 | ... | 1 35 | 2 0 | 1 70 | 2 3 | 1 80 | 2 3 | 1 90 | 2 3 | 0 87 | 1 6 |
| September... | 3 80 | ... | 3 41 | 4 5 | 3 60 | 4 4 | 3 90 | 4 5 | 3 40 | 3 5 | 1 10 | 3 0 |
| October ...... | 4 00 | ... | 4 00 | 4 3 | 4 50 | 5 5 | 4 30 | 5 5 | 4 30 | 5 5 | 2 77 | 3 8 |
| November ... | 3 20 | ... | 3 15 | 3 8 | 4 30 | 4 5 | 3 80 | 4 5 | 3 90 | 4 5 | 1 55 | 2 4 |
| December ... | 4 20 | ... | 3 80 | 5 2 | 6 30 | 11 0 | 6 20 | 11 0 | 6 50 | 13 8 | 4 63 | 3 0 |
| | 29 38 | ... | 26 95 | 31 3 10 mo. | 35 75 | 47 8 | 34 57 | 48 1 | 34 97 | 46 3 11 mo. | 20 31 | 26 9 |

On the Peak Forest and Ashton Canals, there are only two places where gauges remain on the tops of houses. These are at Marple and at Comb's Reservoir, both on the Peak Forest Canal.

The comparative results between the old gauges placed on the tops of the houses, and the Society's gauges placed on the ground, are as follow:—

| 1845. | Marple, 531 feet. | | Comb's Reservoir, 850 feet. | |
|---|---|---|---|---|
| | Old Gauge. | Society's Gauge. | Old Gauge. | Society's Gauge. |
| | In. Dec. | In. Dec. | In. Dec. | In. Dec. |
| January | 1 04 | 2 20 | 1 68 | 3 70 |
| February | 0 48 | 2 50 | 0 79 | 1 80 |
| March | 1 01 | 1 70 | 2 00 | 3 60 |
| April | 1 36 | 3 00 | 1 68 | 3 00 |
| May | 0 71 | 1 45 | 1 85 | 2 70 |
| June | 2 82 | 4 25 | 3 57 | 5 00 |
| July | 3 50 | 4 75 | 3 97 | 5 10 |
| August | 5 31 | 8 25 | 7 52 | 9 10 |
| September | 1 92 | 2 75 | 4 00 | 5 00 |
| October | 1 94 | 2 70 | 2 75 | 4 10 |
| November | 1 20 | 1 75 | 2 36 | 4 00 |
| December | 2 28 | 3 50 | 4 73 | 8 00 |
| | 23 57 | 38 80 | 36 90 | 55 10 |

Mr. Wood, the Canal Company's engineer, in whose charge the gauges are placed, is of opinion that the Society's gauge at Comb's Reservoir for this year (1845) indicates a greater quantity of rain than was received by the gauge, as he found it to be leaky on removing it to a more convenient position. The returns do not show, apparently, any greater discrepancy than is to be observed at other places where no leakage occurred.

| 1846. | Marple, 531 feet. Old Gauge. | Marple, 531 feet. Society's Gauge. | Comb's Reservoir, 850 feet. Old Gauge. | Comb's Reservoir, 850 feet. Society's Gauge. |
|---|---|---|---|---|
| | In. Dec. | In. Dec. | In. Dec. | In. Dec. |
| January | 2 81 | 4 15 | 3 23 | 5 50 |
| February | 0 54 | 0 80 | 1 20 | 2 00 |
| March | 1 16 | 1 60 | 1 91 | 2 60 |
| April | 3 46 | 5 00 | 4 10 | 4 60 |
| May | 0 60 | 0 85 | 1 49 | 2 20 |
| June | 1 80 | 2 45 | 2 62 | 2 70 |
| July | 2 37 | 3 25 | 2 48 | 2 30 |
| August | 2 52 | 3 35 | 4 45 | 5 30 |
| September | 0 94 | 1 45 | 1 02 | 1 20 |
| October | 5 11 | 6 35 | 4 36 | 5 10 |
| November | 1 23 | 1 70 | 1 89 | 2 50 |
| December | 1 03 | 1 40 | 1 18 | 2 10 |
| | 23 57 | 32 35 | 29 93 | 38 10 |
| **1847.** | | | | |
| January | 1 20 | 1 65 | 1 58 | 2 60 |
| February | 2 18 | 3 00 | 2 28 | 3 50 |
| March | 0 75 | 1 05 | 1 14 | 1 60 |
| April | 2 30 | 3 00 | 2 35 | 4 00 |
| May | part only. 1 23 | 2 80 | 4 15 | 5 30 |
| June | 2 57 | 4 10 | 2 88 | 3 90 |
| July | 1 33 | 1 80 | 0 93 | 1 00 |
| August | 2 50 | 3 80 | 3 48 | 3 40 |
| September | 4 22 | 7 90 | 5 74 | 8 20 |
| October | 2 71 | 5 50 | 3 62 | 5 40 |
| November | 2 22 | 4 10 | 3 18 | 4 50 |
| December | 3 16 | 7 00 | 4 24 | 7 90 |
| | 26 37 | 45 70 | 35 57 | 51 30 |

Accompanying the above returns from the Peak Forest and Ashton Canals, there have also been received the returns of the rain which has fallen at other places on or near the lines of the Canal, as indicated by gauges which have always been placed near the level of the ground. They are as follow:—

| 1845. | Fairfield, 320 feet. Old Gauge, a few feet above Ground. | Fairfield, 320 feet. New Gauge, 1 foot above Ground. | Water-houses Lock, Ashton Canal, 360 feet above sea. | Inclined Plane, Chapel-le-Frith, 1121 feet. |
|---|---|---|---|---|
| January | 2 20 | 1 80 | 1 90 | 2 60 |
| February | 1 60 | 1 70 | 2 00 | 2 00 |
| March | 3 60 | 3 00 | 3 20 | 2 20 |
| April | 1 90 | 2 20 | 2 40 | 2 80 |
| May | 1 30 | 1 10 | 1 50 | 2 30 |
| June | 4 20 | 3 70 | 3 90 | 4 00 |
| July | 3 60 | 3 40 | 3 80 | 3 90 |
| August | 7 30 | 6 70 | 6 40 | 7 60 |
| September | 3 00 | 2 80 | 2 70 | 3 60 |
| October | 3 70 | 3 70 | 4 00 | 3 10 |
| November | 3 00 | 2 80 | 3 00 | 3 10 |
| December | 7 10 | 6 00 | 8 00 | 6 00 |
| | 42 50 | 38 90 | 42 80 | 43 20 |
| 1846. | | | | |
| January | 3 60 | 3 30 | 3 90 | 4 40 |
| February | 1 30 | 1 30 | 1 40 | 1 60 |
| March | 1 70 | 1 70 | 1 80 | 2 60 |
| April | 3 00 | 3 00 | 3 50 | 5 30 |
| May | 0 90 | 0 90 | 0 80 | 1 90 |
| June | 2 90 | 2 40 | 2 80 | 4 00 |
| July | 3 90 | 3 90 | 4 20 | 2 90 |
| August | 3 60 | 3 60 | 3 80 | 5 00 |
| September | 0 80 | 0 80 | 0 70 | 1 30 |
| October | 5 30 | 4 70 | 4 90 | 5 40 |
| November | 3 70 | 3 10 | 2 40 | 2 40 |
| December | 2 10 | 1 50 | 1 60 | 2 00 |
| Total | 32 80 | 30 20 | 31 80 | 38 80 |

For the year 1847, returns have been received of the rain which has fallen at various places within the district traversed by the Peak Forest and Macclesfield Canals, and the Manchester, Sheffield and Lincolnshire Railway, in addition to those already alluded to. Many of these are of an experimental nature. Those which appear to bear upon

the subject of this paper are introduced in the following table :—

| 1847. | Fairfield, 320 feet above Sea. | | Waterhouses Lock, 360 feet. | Inclined Plane, Chapel-le-Frith, 1121 feet. | Todd's Brook Reservoir, 620 ft. | Brink's Edge, 1500 feet. | Comb's Moss, 1670 feet. | Bosley Reservoir, M'clesfield Canal, about 600 feet. | Woodhead Station, Manchester and Sheffield Railway, 1000 ft. |
|---|---|---|---|---|---|---|---|---|---|
| | Old Gauge. | New Gauge. | | | | | | | |
| January ....... | 1 50 | 1 50 | 1 20 | 2 80 | 1 78 | 1 14 | 2 60 | ... | 2 20 |
| February....... | 2 90 | 3 00 | 2 00 | 3 90 | 2 33 | 1 68 | 1 78 | ... | 2 64 |
| March .......... | 0 80 | 0 80 | 1 20 | 1 30 | 0 88 | 0 77 | 0 79 | 1 05 | 1 84 |
| April............ | 2 20 | 3 30 | 2 80 | 3 90 | 3 08 | 1 93 | 2 45 | 2 58 | 3 21 |
| May ............. | 4 95 | 5 10 | 5 30 | 4 50 | 4 17 | 3 95 | 4 50 | 3 91 | 3 16 |
| June ............ | 3 20 | 3 30 | 3 10 | 3 20 | 3 70 | 1 05 | 3 44 | 3 18 | 2 62 |
| July ............. | 1 00 | 0 95 | 0 80 | 1 50 | 0 97 | 0 95 | 1 53 | 1 43 | 1 29 |
| August ......... | 4 00 | 3 70 | 3 30 | 2 90 | 2 95 | 2 40 | 2 96 | 4 77 | 1 57 |
| September .... | 5 50 | 5 50 | 5 50 | 6 40 | 6 17 | 4 40 | 4 17 | 6 35 | 2 98 |
| October......... | 4 90 | 4 80 | 4 70 | 4 30 | 3 71 | 2 77 | 4 48 | 2 97 | 4 72 |
| November .... | 3 50 | 3 50 | 3 80 | 3 60 | 3 05 | 3 35 | 2 52 | 2 84 | 2 45 |
| December...... | 5 20 | 5 30 | 4 60 | 5 70 | 5 60 | 5 05 | 4 63 | 3 95 | 4 44 |
| | 39 65 | 40 75 | 38 30 | 44 00 | 38 39 | 29 44 | 35 85 | 33 03 10 mos. | 33 12 |

The gauges employed at the five new places in the preceding table, viz., Todd's Brook Reservoir, Brink's Edge, Comb's Moss, Bosley Reservoir, and Woodhead, are all similar, and of a new construction. The results indicated by them vary so greatly from those of other gauges in their immediate neighbourhood, as to occasion great suspicion of their accuracy. For instance, the gauge at Woodhead, which is situated at the head of the Longdendale Valley, hereafter alluded to, shows only 33 inches of rain at an elevation of 1000 feet above the sea. From this district it was ascertained by careful daily measurement, that 49½ inches of water had actually flowed off the ground, and the rain indicated by two neighbouring gauges in the same district, within a distance of about 2½ miles, was, respectively,

$50\frac{1}{2}$ inches and $62\frac{1}{10}$ inches—the latter being at an elevation of about 1750 feet, and the former 700 feet above the sea.

It seems exceedingly probable, from an examination of this new gauge, that it is liable to be affected in a serious degree by evaporation, and the difference may perhaps be assigned to this cause. The whole apparatus is placed above the surface of the ground—the water is received by a metal funnel, and conducted from thence by a short pipe at the bottom, through the top of a wooden box, to a glass bottle of large area placed within the box and open at the top. The water is not covered by a float, and the surface is therefore in contact with the atmospheric air. It is not suggested that the evaporation would be as great as from the surface of an open pond; but that it does take place to a considerable degree seems to be evident.

As evaporation is greater in proportion to the altitude of the situation, the supposition that the discrepancy is to be assigned to this cause, will also account for the anomaly (as compared with the general result of all the other returns), at Todd's Brook and Comb's Moss, where it would appear, from the results furnished by these new gauges, that *less* rain falls on high land than on low. If the returns were properly corrected by a due allowance for evaporation according to the altitude, the true state of the case would probably be found to agree with the evidence from other places.

The rain gauge at the top of the inclined plane at Chapel-le-Frith is an exception to this rule, as it appears to show, pretty regularly, less rain than falls at Comb's Reservoir at a lower elevation. This may probably arise from local causes, as it is at variance with the general testimony within the range of the same elevation.

All the Society's gauges,—those at Fairfield, and that at the top of the inclined plane at Chapel-le-Frith, put down by the Canal Company,—are cylindrical gauges, with an

upright graduated rod attached to a float covering the surface of the water, which indicates the depth of rain caught within the cylinder.

Objections have been made to this form of gauge from the effect alleged to be produced by the rod as it rises above the level of the top of the cylinder, exposing additional surface, and adding in that manner to the collecting surface of the gauge.

Experiments have been made for some years by Mr. Wood, the engineer to the Ashton and Peak Forest Canals, to ascertain the effect produced by rods standing some height above the top of the gauge.

That a sensible effect is produced, increasing the quantity of water caught, appears to be clearly established from these experiments; but they do not afford any assistance in determining the height at which the rod begins to affect the accuracy of the register, nor do they furnish any data from which to calculate the proportionate increased quantity due to the elevation of the rod.

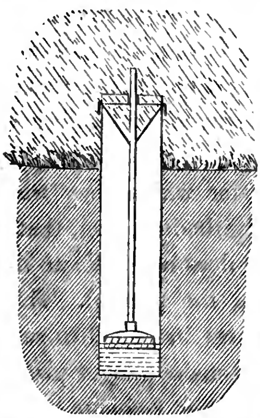
For instance, in one situation a rod of 1 inch in diameter, standing 24 inches above the top of the gauge, collected in 12 months 3·18 inches of rain; while in another place a rod of the same diameter, but only 18¾ inches long, collected in the same time 27·89 inches, the quantity of rain as indicated by the rain gauges being respectively 40·75 and 51·30.

Again, a staff or rod of 2 inches diameter, and 18¾ inches long, collected in 12 months 20·67 inches, the rain being apparently at that place 38·39. At another place, a rod of the same diameter and length, where the rain appears to have been 35·85, collected in the same period 58·99 inches.

These differences are enormous, and apparently unaccountable.

On consideration, it appears probable that the rod will produce no sensible effect until it rises to a height greater

than half of the diameter of the gauge. This supposes that the rain will not often reach the ground at a more acute angle than 45°. If the rain descend at that angle, and the rod stand at a height equal to half the diameter of the gauge, any rain which would be intercepted by the rod would have fallen, had the rod not been there, within the area of the top of the gauge, as will be seen by the sketch in the margin.



If the rain come straight down, or nearly so, as it often does in heavy rain in summer, the rod might stand at a much greater height without producing any effect. If the rain be driven by strong wind nearly horizontally, which would be the case in very exposed situations, then of course a slight elevation of the rod will intercept rain which would otherwise pass entirely over the gauge. It may, however, be fairly assumed, that in the foregoing observations from the Society's gauges, which are about 7 inches in diameter, the results may be taken as perfectly accurate till the rod rises more than 3½ inches above the top. In taking the observations the gauges have been regularly emptied at the end of every month; and there would only be some amount of error, therefore, in those months in which the rain exceeded 3½ inches, the amount of error being probably proportionate to the greater depth of rain.

To remedy this objection for the future, instructions have been given either to tie down the rods, or to detach them from the float, merely using them at the time an observation is taken. The latter method is the best, as the float rises on the surface of the water collected, thereby preventing evaporation; while by tying down the rod, the water would rise above the float, and be subject, to some slight ex-

tent, to loss from so much evaporation as could take place on the surface of the water confined within the cylinder and covered by the funnel top, the hole at the bottom of which is almost entirely filled by the graduated rod. This could not be very much; but to whatever extent it did take place, it would affect the result and show less than the real fall.

Notwithstanding the objections which may be made to the precise accuracy of the observations, in consequence of the effect produced by the index rod having been allowed to rise in some months to a height greater than three or four inches, and that they may be supposed therefore to indicate a fall of rain something greater than the truth, they have fully served the purpose for which they were undertaken. They have clearly established the fact, that a gauge placed on the top of a house does not indicate the correct quantity of water falling on the ground. They also show that a large per centage must be added to such observations as have been made by means of gauges placed on buildings, in order to form any correct estimate of the actual fall of rain at the place of observation; and they may in that way be made materially useful in correcting the results of many years of observations in such unfavourable positions.

They show also, that within the range of the observations more rain falls on high ground than on low.

The observations which are now being made, will, it is hoped, be free from all objections; and if future results confirm those of the past, it would be desirable to recommend to all parties who have hitherto been at the trouble of keeping registers of the quantity of rain falling in different places, but whose gauges have been injudiciously placed, to continue them for the future in situations better calculated for obtaining accurate results.*

* Since this paper was written, the author has received the Annual Report for 1847 of Mr. J. F. Miller of Whitehaven, printed for private circulation amongst the subscribers to the very valuable and interesting meteorological

The following observations upon the fall of rain and the quantity of water flowing off the ground, are from measurements taken in the course of the enquiries made during the last few years, with reference to the supply of Man-

observations conducted by him. It contains the following judicious and important remarks in reference to the points just alluded to:—

"In the last Report I observed, 'it would be premature, from the scanty data before me, to draw any conclusion as to the gradation in the quantity of rain at the great elevations above the sea. But it seems probable that, in mountainous districts, the amount of rain increases from the valley upwards, to an altitude of about 2000 feet, where it reaches a maximum; and that above this elevation the quantity rapidly decreases.'

"The Table for 1846, exhibited the rain fall of the summer months only, but the additional returns of 1847, obtained in every variety of season, confirm the above deductions in every essential particular; so that we may fairly assume the combined results to be indicative of a physical law, so far, at least, as relates to the particular locality in question. Thus in twenty-one months,

| | | | | | | |
|---|---|---|---|---|---|---|
| The Valley ......... | 160 | feet above sea, | has received | 170·55 | inches. |
| „ | Stye-Head...... | 1290 | „ | „ | 185·74 | „ |
| „ | Seatollar ...... | 1334 | „ | „ | *180·28 | „ |
| „ | Sparkling Taru | 1900 | „ | „ | 207·91 | „ |
| „ | Great Gabel ... | 2925 | „ | „ | 136·98 | „ |
| „ | Sca Fell ...... | 3166 | „ | „ | 128·15 | „ |

"An apparent exception to this law occurs at the gauge stationed at Burnt Rigg, about midway between the top of Stye-Head and the vale of Wast dale, and which in 1847 has received about one-eighth, or twelve and three quarters per cent., *less* rain than the valley.

"This is the only one of the gauges situated on the slope of a mountain; it is on the windward side, and I imagine that in such a position, eddies or counter-currents are produced in windy weather which cause a less quantity of water to be deposited in the instrument than is due to the elevation. We know that all sloping roofs, from the same cause, materially diminish the receipts of rain gauges.

"It will be observed that the amount of water received by the Seatollar gauge, is invariably less than the deposit in the adjacent vale of Seathwaite, and the deficiency is pretty equable in every month of the year.

"I am unable to give any satisfactory reason for this anomaly, or to

* The height of Seatollar common has not been accurately ascertained.

chester with water from the hills beyond Stalybridge and Mottram—lying at a distance of from ten to twenty miles east of Manchester.

In the highest part of this range of hills, known by the name of the Penine Chain, the river Etherow and its various mountain tributaries take their rise. Some of these uniting near Woodhead, form theré a deep romantic valley

account for the very great excess of rain in this valley over all others in the Lake districts. As the gauge on Seatollar is two or three miles distant in a direct line from the others, the near approach of its receipts to those of the Stye-Head gauge, both about the same elevation, is rather remarkable. In 1846 the Seatollar exceeded the Stye gauge in quantity, which it should do if the assumed height be correct.

"By referring to the table for the summer months, we find that between the 1st of May and the 31st of October, the gauge at 1290 feet has obtained nearly twelve per cent. more rain than the valley; at 1334 feet, nine and a half per cent. more; at 1900 feet, twenty-nine per cent. more; at 2928 feet, seven and a half per cent. less; and at 3100 feet, thirteen and a half per cent. less than the valley. In the winter months (November to April inclusive) the gauge at 1290 feet has received four and a half per cent. more than the valley; at 1334 feet, the same quantity as the valley; at 1900 feet, eleven and a quarter per cent. more; at 2928 feet, thirty-eight and a half per cent. less, and at 3100 feet, forty-two and a half per cent. less than the valley.

"The difference in the proportion to the valley between the winter and summer half-year, as shown by the tables, is rather startling.

"When much snow falls, no doubt a considerable portion is lost to the gauge, either by its being blown out of the funnel, or by the aperture getting choked up. But I do not think that this cause alone is at all adequate to account for the great comparative deficiency in the winter season; for there was very little snow on the mountain tops during the winter 1846-7, less, I am told by one of the oldest inhabitants of the Fell dales, than he almost ever remembers. At Whitehaven, we had no snow worth naming, except on the night of the 23rd of December, where it lay to the depth of nearly an inch on the ground, but disappeared in course of the ensuing morning.

"The late Mr. Crosthwaite of Keswick, by means of marks on the side of Skiddaw, and with the assistance of a telescope at his residence, made two or three daily observations on the heights of clouds for several years; and it is clearly shown by his tables, that the clouds are lowest in the

called Longdendale, running for several miles nearly due west between hills which rise abruptly on each side to a considerable height, reaching in some cases nearly 2000 feet above the level of the sea.

The valley is hemmed in to the west by the high land at Mottram, which, however, is not high enough to intercept the clouds driven before the westerly winds.

three first and three last months of the year. Moreover, Dr. Dalton affirms that the clouds are seldom a mile high (or little more than one and a half times the height of Sca Fell), in our climate, in winter. Now the Doctor here probably alludes to, or at least includes, the most elevated clouds, such as the Cirri, and some variety of the Cirrostratus. But there can be no doubt, that between the months of November and March the under surface of the Nimbus or rain cloud (the lowest except the Stratus) is far below the tops of our highest mountains, and, I have reason to believe, not unfrequently its upper surface also; when this is the case, the gauges on Sca Fell, Gabel, &c., will receive no rain at all, when it is descending abundantly in the valleys beneath. The lowness of the rain-cloud at this season, is, I apprehend, the principal cause of the small quantity of rain in proportion to the valley, during the winter as compared with the summer months."

He also observes as to the value of the experiments, that "they have already shown us that at least sixty inches more rain is deposited in England than we were previously aware of:—that one hundred and fifty inches sometimes descends in the Lake districts in a year,—more than falls in most parts of the Tropics with which we are acquainted, and sufficient to drown standing two of the tallest men in Great Britain, one on the top of the other. They have further informed us, that six and a half perpendicular inches of water is sometimes precipitated from the atmosphere in twenty-four hours, and ten inches in forty-eight hours; a quantity which would be thought large for any two consecutive months in most parts of England. We have further ascertained that the almost incredible depth of thirty inches occasionally descends in a single month—a fall nearly equal to the calculated yearly average for all other parts of England. The experiments have, in short, enabled us to collect a number of new and curious facts, bearing on the quantity and very unequal distribution of rain in this island. We have also ascertained with a high degree of probability, the law of the gradation in the amount of rain, at various intermediate points, between the valleys and the tops of the highest mountains.

In the upper part of the valley the tributary streams, falling from 1000 to 1200 feet in a few miles, join the main stream in the valley of Longdendale, nearly at right angles, thus breaking the surface of the country into various cross valleys and deep ravines. The summit of this district is Holme Moss, nearly 2000 feet above the sea. It is the highest eminence in the whole chain, though it rises but

A little consideration will greatly lessen our surprise at the enormous quantity of water deposited in the hilly districts of Cumberland and Westmoreland, and at the consequent unequal distribution of the rain in the climate of Great Britain. To those unacquainted with these localities it may be briefly stated, that the Lake District valleys radiate from a series of mountains of slate and primitive rock, having the Gabel, 2928 feet in height, as a nucleus or central point; and in the immediate vicinity of which are Sca Fell and Pillar, of the respective elevations of 3166 and 2893 feet, and Great End, Bowfell, and Glaramara, not much inferior in altitude. These mountains are distant only about thirteen or fifteen miles in a direct line from the Irish Channel, and, as no hills intervene, they are consequently fully exposed to our wet and prevailing winds, which are the south-west. The warm south-westerly current arrives at the coast loaded with moisture, obtained in its transit across the Atlantic :—Now, our experiments justify us in concluding, that this current has its maximum density at about 2000 feet above the sea level; hence, it will travel onward until it is obstructed by land of sufficient elevation to precipitate its vapour; and, retaining a portion of the velocity of the lower parallel of latitude whence it was originally set in motion, it rapidly traverses the short space of level country, and with little diminution of its weight or volume; but, on reaching the mountains, it meets with a temperature many degrees lower than the point at which it can continue in a state of vapour;—sudden condensation consequently ensues, in the form of vast torrents of rain, which in some instances must descend almost in a continuous sheet, as when nine or ten inches are precipitated in forty-eight hours. When we reflect that a warm moist current, perhaps only 3° or 4° above the point of saturation, in coming in contact with the mountain ridge, probably meets with a stratum of air 10° or 15° lower than its own inherent temperature, we shall cease to marvel that such quantities as four, five, or even six inches of water should be deposited in these localities in the course of a few hours. The mountains are, in fact, huge natural condensers, destined to force from the atmosphere the mighty volumes of water requisite for the supply of our lakes and rivers."

slightly above the surrounding table land, the elevation of which, from the crests of the valleys just described, varies from 1500 to 1900 feet.—The millstone grit caps the summits in various parts, forming occasionally perpendicular precipices several hundred feet in height; and in other parts the sides of the valleys consist of beds of indurated shale. Some considerable portions of the tops of the hills are covered with peat and others with gravelly clay. The more easterly portion of the district consists of the lower coal measures.

For hills of this elevation it is scarcely possible to find any which, from their position and character, would be more likely to induce a large fall of rain, or to allow a larger proportion of that which falls to flow down the streams. It is from this district that the town of Manchester is to be supplied with water.

Measurements of the volume of all the various streams have been made daily since the end of 1846, and rain gauges have been placed at various elevations and at different parts of the district, from which the quantity of rain which has fallen has been ascertained.

In an adjoining valley, down which flows the Swineshaw Brook, a tributary of the river Tame, similar observations have been made since the end of 1844. This valley lies nearly east and west, and is completely land-locked, turning abruptly to the north through a narrow glen just before it joins the river Tame. The summit of the valley is at Windyate Edge, which is the summit also of two tributaries of the Etherow, the Hollingworth and Arnfield brooks.

Rain gauges were placed at the bottom of the Swineshaw valley, near the point at which the volume of the stream was measured, and on Windyate Edge near the summit. For some time also a gauge was kept about midway.

The brook was measured regularly twice a day. During the years 1846 and 1847, the index rod of the rain gauges

was constantly tied down so as to prevent its rising above the top of the cylinder—and partially so during 1845. These observations therefore are free from objection on account of the additional surface exposed by the rod, though there may have been some loss from evaporation.

In taking the rain-gauge observations in the Longdendale district, the index rod was tied down from their commencement in November 1846 to Midsummer 1847, since which time the rod has been detached, and inserted only at the time an observation is being taken.

The streams were measured, one (the river Etherow) three times a day, some twice a day, and others once a day. But in the results, which are given in monthly amounts, it has been found necessary to omit many months, in consequence of the gauges being frequently injured and rendered unfit for use for some days, by the effects of violent and destructive floods.

## SWINESHAW BROOK.

### RAIN, AND DEPTH OF WATER FLOWING OFF GROUND.

The extent of ground draining to the point at which the volume of tho stream is measured is 1250 statute acres.

| | Rain. | | | | Flow. |
|---|---|---|---|---|---|
| 1845. | Brushes, 480 feet above the Sea. | Windyate Edge, 1700 feet. | Bower Flat, 1400 feet. | Probable mean rain over district. | Depth in inches flowing off ground. |
| | In. Dec. | In Dec. | In. Dec. | In. Dec. | In. Dec. |
| January | — | 3 1 | — | 3 0 | 1 668 |
| February | — | 1 8 | 1 5 | 1 8 | 1 764 |
| March | — | 5 5 | 4 0 | 5 5 | 2 892 |
| April | 2 5 | — | 2 7 | 3 0 | 2 424 |
| May | 2 8 | — | 3 6 | 3 6 | 2 436 |
| June | 4 3 | — | 4 1 | 4 3 | 2 364 |
| | | | | 21 2 | 13 548 |
| July | 4 2 | 4 7 | 5 0 | 4 7 | 3 048 |
| August | 8 3 | 14 8 | 9 6 | 10 0 | 7 236 |
| September | 3 3 | 4 1 | 3 2 | 3 7 | 2 736 |
| October | 4 9 | 5 9 | — | 5 5 | 4 608 |
| November | 3 4 | 4 0 | — | 3 7 | 2 904 |
| December | 10 3 | 11 0 | — | 11 0 | 6 624 |
| | | | | 38 6 | 27 156 |

Depth of rain in first six months 21·2 inches, of which there flowed off the ground 13·548, or nearly two-thirds.

Depth of rain in last six months 38·6 ins., of which there flowed off the ground 27·156 ins., or nearly three-fourths.

Rain for the whole year 59·8 ins., of which there passed down the brook 40·704, or upwards of two-thirds.

It is possible that in this year the fall of rain has been registered too high, in consequence of comparative inattention to the index rod.—The fall of rain in other places was just an average.

SWINESHAW BROOK.—1846.

| 1846. | Rain. Brushes, 480 feet above the sea. | | Windyate Edge, 1700 feet. | | Probable Mean. | | Flow. Depth in inches flowing off ground. | |
|---|---|---|---|---|---|---|---|---|
| | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. |
| January | 3 | 8 | 6 | 8 | 5 | 3 | 4 | 908 |
| February | 2 | 0 | 2 | 2 | 2 | 1 | 3 | 180 |
| March | 2 | 1 | 2 | 2 | 2 | 2 | 1 | 944 |
| April | 5 | 9 | 4 | 8 | 5 | 3 | 4 | 894 |
| May | 1 | 4 | — | | 1 | 5 | 1 | 452 |
| June | 4 | 0 | 5 | 8 | 5 | 0 | 1 | 176 |
| | | | | | 21 | 4 | 17 | 554 |
| July, August, September | 10 | 0 | | | 11 | 0 | | |
| October | 5 | 3 | 5 | 6 | 5 | 4 | 3 | 636 |
| November | 2 | 4 | 2 | 4 | 2 | 4 | 2 | 184 |
| December | 2 | 1 | 2 | 7 | 2 | 4 | 2 | 868 |
| | | | | | 21 | 2 | 8 | 688 |
| First six months | | | | | 21 | 4 | 17 | 554 |
| | | | | | 42 | 6 | 26 | 242 |
| Say for three months omitted | | | | | | | 7 | 000 |
| | | | | | | | 33 | 242 |

In this year the index rod of the rain gauge was tied down. The measurement of the stream was suspended during July, August, and September; but it is probable that the 7 inches supposed to have flowed off the ground during this period is not far from the truth. This year was considerably below the average fall of rain: nearly one of the driest on record; and the above results may be taken as the fall and produce in such an extreme period.

SWINESHAW BROOK.—1847.

| 1847. | Rain. Brushes. | | Rain. Windyate Edge. | | Rain. Probable Mean. | | Flow. Depth in inches flowing off ground. | |
|---|---|---|---|---|---|---|---|---|
| January | 1 | 7 | 1 | 8 | 1 | 75 | 2 | 1 |
| February | 4 | 4 | 3 | 9 | 4 | 15 | 4 | 8 |
| March | 1 | 7 | 1 | 4 | 1 | 55 | 1 | 7 |
| April | 4 | 0 | 5 | 1 | 4 | 55 | 3 | 2 |
| May | 6 | 8 | 8 | 0 | 7 | 40 | 4 | 4 |
| June | 3 | 5 | 3 | 7 | 3 | 60 | 2 | 0 |
| | 22 | 1 | 23 | 9 | 23 | 00 | 18 | 2 |
| July | 1 | 6 | 1 | 5 | 1 | 55 | 1 | 3 |
| August | 3 | 2 | 3 | 3 | 3 | 25 | 1 | 0 |
| September | 6 | 5 | 6 | 0 | 6 | 25 | 3 | 3 |
| October | 3 | 5 | 4 | 9 | 4 | 20 | 3 | 2 |
| November | 3 | 8 | 5 | 0 | 4 | 40 | 4 | 0 |
| December | 5 | 9 | 7 | 5 | 6 | 70 | 6 | 1 |
| | 24 | 5 | 28 | 2 | 26 | 35 | 18 | 9 |
| Whole Year | 46 | 6 | 52 | 1 | 49 | 35 | 37 | 1 |

The fall of rain during this year was about an average, in some places rather more. It fell, however, very unequally, the last three or four months making up for previous deficiency.

The proportion of the water flowing off the ground to that which fell, was about 3 to 4. The quantity of the rain-fall which was lost to the river, was about 12 ins.: that being apparently the annual amount required for evaporation, the supply of vegetation, and absorption by the ground, in a year of average rain.

The springs in this valley are very copious; and though neither the fall of rain, nor the average volume of the stream, are equal to the Longdendale district, yet the supply

of spring water in dry weather is greater in proportion to the extent of drainage ground.

In December, 1844, no rain fell, and yet the springs yielded a quantity of water equal to a depth of 1 inch over the whole drainage ground, the mean volume of the stream being the same as in August, 1847, in which month the fall of rain was $3\frac{1}{4}$ inches.

The following table shows the fall of rain for the year 1847, and for the two last months of 1846, at all the places in the district at which rain gauges have been put down.

| 1846. | Brushes, 480 feet. | | Wind-yate Edge, 1700 feet. | | Crow-den Hall, 700 feet. | | Rakes Moss, 1620 feet. | | Butterly Moss, 1750 feet. | | Mean of all the Obser-vations. | | Mean, omitting Brushes. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. |
| November ......... | 2 | 4 | 2 | 4 | 2 | 0 | 2 | 4 | 3 | 0 | 2 | 4 | 2 | 4 |
| December ......... | 2 | 1 | 2 | 7 | 2 | 8 | 3 | 1 | 4 | 0 | 2 | 9 | 3 | 1 |
| Two months ....... | 4 | 5 | 5 | 1 | 4 | 8 | 5 | 5 | 7 | 0 | 5 | 3 | 5 | 5 |
| 1847. | | | | | | | | | | | | | | |
| January ............ | 1 | 7 | 1 | 8 | 2 | 2 | 2 | 4 | 3 | 7 | 2 | 4 | 2 | 5 |
| February ......... | 4 | 4 | 3 | 9 | 4 | 3 | 4 | 6 | 4 | 3 | 4 | 3 | 4 | 3 |
| March ............ | 1 | 7 | 1 | 4 | 1 | 7 | 2 | 3 | 1 | 4 | 1 | 7 | 1 | 7 |
| April ............... | 4 | 0 | 5 | 1 | 4 | 7 | 3 | 3 | 9 | 0 | 5 | 2 | 5 | 5 |
| May ............... | 6 | 8 | 8 | 0 | 4 | 9 | 7 | 9 | 4 | 8 | 6 | 5 | 6 | 4 |
| June ............... | 3 | 5 | 3 | 7 | 3 | 1 | 3 | 3 | 3 | 4 | 3 | 4 | 3 | 4 |
| Six months ....... | 22 | 1 | 23 | 9 | 20 | 9 | 23 | 8 | 26 | 6 | 23 | 5 | 23 | 8 |
| July ............... | 1 | 6 | 1 | 5 | 1 | 4 | 2 | 0 | 1 | 1 | 1 | 5 | 1 | 5 |
| August ............ | 3 | 2 | 3 | 3 | 3 | 5 | 5 | 1 | 6 | 5 | 4 | 3 | 4 | 6 |
| September ......... | 6 | 5 | 6 | 0 | 7 | 5 | 8 | 7 | 8 | 2 | 7 | 4 | 7 | 6 |
| October ............ | 3 | 5 | 4 | 9 | 5 | 1 | 4 | 4 | 5 | 4 | 4 | 7 | 4 | 9 |
| November ......... | 3 | 8 | 5 | 0 | 6 | 0 | 4 | 3 | 8 | 3 | 5 | 5 | 5 | 9 |
| December ......... | 5 | 9 | 7 | 5 | 6 | 1 | 8 | 2 | 6 | 0 | 6 | 7 | 6 | 9 |
| Six months ........ | 24 | 5 | 28 | 2 | 29 | 6 | 32 | 7 | 35 | 5 | 30 | 1 | 31 | 4 |
| Whole year ...... | 46 | 6 | 52 | 1 | 50 | 5 | 56 | 5 | 62 | 1 | 53 | 6 | 55 | 2 |

In the preceding table, the last column, which is the mean of all the rain observations, omitting Brushes on account of its being in another valley, may be taken as the mean fall of rain for the year 1847 in the Longdendale district, so far at least as the gauges may be supposed to indicate the real quantity.

The measurements of the streams, however, lead to the belief that more rain has fallen than the rain gauges show. They are placed in the valleys, and at mean heights, none being quite on the tops of the hills; and it is probable that *there* heavy rain has fallen, and contributed to swell the streams, which has been beyond the range of the rain gauges.

The next table exhibits the depth of water flowing off the ground, as measured in various streams in the Longdendale valley. It shows also the drainage or collecting ground to the point of measurement on each stream, and the mean flow as deduced from all the observations.

## LONGDENDALE VALLEY.

DEPTH OF WATER FLOWING OFF THE GROUND.

| 1847. | Hollingworth Brook. Drainage Ground, 1390 acres. | Arnfield Brook. Drainage Ground, 884 acres. | Hollins Brook. Drainage Ground, 470 acres. | Great Crowden Brook. Drainage Ground, 1751 acres. | Little Crowden Brook. Drainage Ground, 1436 acres. | River Etherow, at Vale House. Drainage Ground, 15,676 acres. | Mean Flow. | Mean Rain. |
|---|---|---|---|---|---|---|---|---|
| | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. |
| Jan......... | ... | ... | ... | 3 5 | 3 0 | ... | 3 25 | 2 5 |
| Feb. ........ | ... | ... | ... | ... | 3 4 | ... | 3 40 | 4 3 |
| March ..... | 1 20 | 1 70 | 0 60 | ... | ... | ... | 1 17 | 1 7 |
| April ...... | ... | ... | 4 80 | ... | 4 5 | ... | 4 65 | 5 5 |
| May.... ... | ... | ... | ... | ... | 5 1 | ... | 5 10 | 6 4 |
| June ....... | 1 60 | 1 80 | 0 13 | 2 2 | 2 2 | ... | 1 59 | 3 4 |
| July ........ | 1 90 | 1 08 | 0 24 | 0 7 | 0 7 | ... | 0 92 | 1 5 |
| August ... | 1 06 | 1 41 | 0 78 | 1 8 | 1 4 | ... | 1 29 | 4 6 |
| Sept. ....... | ... | ... | ... | 6 2 | 5 1 | 5 89 | 5 73 | 7 6 |
| October ... | ... | ... | ... | 6 9 | 7 8 | 4 80 | 6 50 | 4 9 |
| Nov. ...,... | ... | 10 40 | 4 90 | 5 6 | 5 7 | 6 90 | 6 70 | 5 9 |
| Dec. ........ | ... | 9 24 | 8 60 | ... | 10 4 | 8 40 | 9 16 | 6 9 |
| | 5 76 | 25 63 | 20 05 | 26 9 | 49 3 | 25 99 | 49 46 | 55 2 |
| Rain during period of observations | 9 95 | 22 42 | 25 80 | 32 65 | 54 7 | 26 10 | | |
| | 4 Mos. | 6 Mos. | 7 Mos. | 7 Mos. | 11 Ms. | 4 Mos. | 12 Ms. | 12 Ms. |

By comparing this Table with that of the Swineshaw brook in the same year, it will be seen that in the Longdendale district the quantity of rain falling and that flowing off the ground are considerably greater.

In the Swineshaw valley the mean rain was 49·35 inches, and the water flowing off the ground 37·1 inches:

In Longdendale the rain was $55\frac{2}{10}$ inches: the produce of which was $49\frac{1}{2}$ inches—*i. e.*, the rain in the two districts is as 49 to 55, the produce as 37 to 49.

By uniting the Swineshaw observations with those in Longdendale, the mean rain and flow would be as follows:—

| 1847. | Rain. | | Flow. | |
|---|---|---|---|---|
| | In. | Dec. | In. | Dec. |
| January | 2 | 36 | 2 | 85 |
| February | 4 | 30 | 4 | 10 |
| March | 1 | 70 | 1 | 30 |
| April | 5 | 22 | 4 | 12 |
| May | 6 | 48 | 4 | 75 |
| June | 3 | 40 | 1 | 65 |
| July | 1 | 52 | 0 | 99 |
| August | 4 | 32 | 1 | 24 |
| September | 7 | 38 | 5 | 12 |
| October | 4 | 66 | 5 | 67 |
| November | 5 | 48 | 6 | 25 |
| December | 6 | 74 | 8 | 55 |
| | 53 | 56 | 46 | 59 |

On examining the last Tables, it will be found that in many months, particularly during October, November, and December, the quantity of water flowing off the ground appears to be larger than the rain which fell during the same period.

During months in which little rain fell, this would be accounted for by the produce of the springs; but in periods of excessive rain, such as the last four months of 1847, in which the rain was $24\frac{1}{4}$ inches, and that which flowed off the ground $25\frac{1}{2}$ inches, although it is reasonable to suppose that the ground would be so saturated that very nearly all the rain would flow down the streams in torrents, yet we could scarcely calculate upon more. The produce of springs from water previously stored up would no doubt add something to the quantity, but not enough to account for the whole.

It is most likely, therefore, that either the streams have been over-estimated, or the rain under-measured.

On a careful examination of all the returns from which the tables have been constructed, it seems probable that the latter supposition is the correct one. Every stream bears the

same sort of evidence, although the measurements were necessarily taken at different periods of the day.

It is true that at all times, and in swollen states of brooks particularly, the measurement of streams by daily gaugings, although they are repeated several times a day, can only be considered as a tolerable approximation to the truth. According to the height of the flood at the time the measurement is taken, they may indicate rather more, or rather less, than the average quantity. Still the observations, regularly continued, will in the course of the year pretty well correct each other, and the result, obtained by taking the mean of upwards of seven hundred measurements of the same stream, at equal intervals of time, cannot be far from the truth.

It is probable that had rain gauges been placed on the summits of the highest hills, as well as in the valleys, and on elevated parts about midway of the whole rise, the returns would have shown a greater fall.

The result of observations made by Mr. Hawksley during the past year in the district surrounding Rivington Pike, near Chorley, from whence the town of Liverpool is proposed to be supplied with water, confirm the accuracy of the observations which have been made in the Longdendale district, the nature of the ground and the character of the hills being very similar in both cases.

Mr. Hawksley found the mean rain, from gauges placed in various parts of the district, varying from 430 feet to 1800 feet above the level of the sea, to be $56\frac{1}{2}$ inches, and the quantity of water which flowed off the ground to be about 44 inches, leaving $12\frac{1}{2}$ inches to supply evaporation, vegetation, and absorption.

The greatest rain-fall was found to be at an elevation of about 1000 feet.

---

On this point some valuable information may be obtained from observations made at the Edinburgh Waterworks.

That city is supplied with water from the Pentland hills. It is mainly collected in a large reservoir called the Glencorse reservoir, about 730 feet above the level of the sea, which affords the means of ascertaining with the greatest accuracy the quantity of water which flows into it from the elevated tract of country above. The mean height of this collecting ground, which consists of 3820 statute acres, is about 1100 feet above the sea, the summits varying from 1300 to 1500 feet.

A rain gauge is kept at the Glencorse reservoir, the mean rain of 1844, 1845, and 1846 being 37·403 inches—the average for sixteen years is 37·067; the maximum (1836) 49·080; the minimum (1842) 25·675 inches.

From the 1st December 1846 to the 31st March 1847, there came into the reservoir a quantity of water equal to a depth over the whole drainage ground of 4·53 inches: the rain during the same period as registered by the rain gauge being 4·35 inches.

This period was remarkably dry in Scotland, the mean of sixteen previous years being 11·442 inches, and the mean of the four lowest 6·150 inches.

Here is an instance, from the most accurate measurement, of the flow of water exceeding for four months the fall of rain. The gauge is certainly placed at the lowest point; but the fall of rain was so small, that a larger proportion than usual would be lost by absorption and evaporation. The fact must be accounted for by supposing that the fall of rain was much heavier in the highest parts of the district, as the springs alone, unswollen by rain, would not have yielded the quantity.

In another more elevated part of the Pentland hills, the Bonally district, the fall of rain from the 1st of December 1846, to the 28th February 1847, was 4·71 inches; the produce 4·55 inches: this was ascertained by repeated gaugings.

Observations are now being carried on by the Bolton Waterworks Company which will throw much light on this subject; but they are not yet in a condition to be laid before the public, though it is hoped, at a future time, when the observations are completed, permission may be given to use them.

The rain at Belmont, in the Bolton Waterworks district, for the last five years, is as follows,—the elevation 850 feet.

| | 1843. | | 1844. | | 1845. | | 1846. | | 1847. | | Mean | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. | In. | Dec. |
| January ...... | 3 | 0 | 6 | 3 | 4 | 1 | 7 | 0 | 1 | 9 | 4 | 4 |
| February ... | 1 | 9 | 5 | 9 | 1 | 9 | 2 | 1 | 3 | 3 | 3 | 0 |
| March......... | 2 | 7 | 4 | 5 | 4 | 0 | 3 | 3 | 1 | 8 | 3 | 2 |
| April ......... | 12 | 0 | 1 | 8 | 2 | 9 | 4 | 4 | 2 | 8 | 4 | 8 |
| May .......... | 4 | 4 | 1 | 9 | 1 | 7 | 1 | 9 | 6 | 3 | 3 | 2 |
| June .......... | 5 | 0 | 3 | 3 | 5 | 2 | 2 | 4 | 6 | 3 | 4 | 4 |
| July .......... | 8 | 0 | 3 | 7 | 3 | 9 | 6 | 6 | 1 | 6 | 4 | 7 |
| August ....... | 4 | 5 | 10 | 0 | 10 | 2 | 5 | 0 | 3 | 7 | 6 | 7 |
| September ... | 1 | 0 | 6 | 4 | 4 | 1 | 2 | 4 | 9 | 7 | 4 | 7 |
| October ...... | 11 | 1 | 3 | 5 | 5 | 0 | 7 | 1 | 7 | 5 | 6 | 8 |
| November ... | 7 | 4 | 2 | 4 | 4 | 0 | 3 | 7 | 8 | 5 | 5 | 2 |
| December ... | 2 | 0 | 0 | 3 | 8 | 0 | 3 | 9 | 8 | 0 | 4 | 5 |
| | 63 | 0 | 50 | 0 | 55 | 0 | 49 | 8 | 61 | 4 | 55 | 6 |

The returns for 1843 and 1844 have been given in former papers, but they are introduced again for the purpose of bringing all together. Here the gauge has been emptied every month, the index rod being allowed to rise during that period, and the returns are therefore liable to the objection before alluded to. The quantities registered may be somewhat too high.

---

By the kindness of Mr. John Ecroyd of Rochdale, the following table of rain which has fallen at that place for the last sixteen years is introduced.

| MONTH. | 1832. | 1833. | 1834. | 1835. | 1836. | 1837. | 1838. | 1839. | 1840. | 1841. | 1842. | 1843. | 1844. | 1845. | 1846. | 1847. | MEAN. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. | In. Dec. |
| January | 1 75 | 0 08 | 9 67 | 5 05 | 5 32 | 5 46 | 0 43 | 4 01 | 5 78 | 2 43 | 4 06 | 5 65 | 5 02 | 3 50 | 5 53 | 2 62 | 4 14 |
| February | 1 02 | 6 40 | 4 50 | 5 94 | 4 37 | 5 20 | 2 38 | 4 28 | 2 11 | 2 14 | 1 50 | 1 32 | 2 20 | 1 87 | 1 95 | 3 75 | 3 18 |
| March | 4 01 | 1 81 | 3 49 | 5 80 | 6 18 | 1 87 | 3 25 | 5 56 | 0 34 | 3 12 | 3 81 | 2 01 | 7 21 | 3 81 | 2 97 | 1 43 | 3 51 |
| April | 2 76 | 3 64 | 1 77 | 1 55 | 3 72 | 1 64 | 2 77 | 1 05 | 0 90 | 1 34 | 0 05 | 7 64 | 1 43 | 3 18 | 4 49 | 3 71 | 2 60 |
| May | 2 61 | 1 00 | 1 10 | 3 03 | 0 25 | 1 67 | 2 67 | 0 43 | 4 45 | 3 89 | 3 25 | 3 88 | 0 21 | 1 84 | 1 73 | 6 42 | 2 40 |
| June | 3 62 | 7 47 | 3 26 | 2 37 | 4 94 | 2 47 | 5 70 | 3 64 | 5 29 | 2 01 | 2 84 | 2 84 | 1 93 | 3 88 | 2 63 | 3 70 | 3 66 |
| July | 2 39 | 4 26 | 6 32 | 2 45 | 7 04 | 3 87 | 5 36 | 5 24 | 5 93 | 6 48 | 4 42 | 5 17 | 4 23 | 4 12 | 5 05 | 1 34 | 4 60 |
| August | 5 73 | 5 18 | 1 60 | 3 30 | 2 73 | 2 21 | 6 92 | 4 92 | 5 59 | 4 98 | 1 94 | 3 94 | 3 41 | 7 17 | 4 32 | 2 15 | 4 13 |
| September | 1 43 | 3 04 | 3 03 | 6 68 | 4 59 | 3 34 | 1 51 | 6 66 | 5 59 | 3 28 | 2 77 | 0 69 | 3 07 | 4 09 | 2 01 | 4 94 | 3 54 |
| October | 5 95 | 4 03 | 3 08 | 5 07 | 4 80 | 5 79 | 8 20 | 3 04 | 2 54 | 6 67 | 3 21 | 9 01 | 2 27 | 5 17 | 5 68 | 6 43 | 5 05 |
| November | 4 92 | 8 75 | 2 60 | 5 27 | 10 35 | 5 64 | 3 35 | 3 82 | 4 78 | 2 69 | 5 90 | 6 82 | 2 84 | 3 35 | 3 01 | 6 83 | 5 05 |
| December | 7 13 | 11 07 | 1 99 | 1 46 | 6 82 | 6 14 | 2 88 | 3 11 | 0 70 | 9 52 | 3 33 | 1 62 | 0 59 | 9 66 | 2 67 | 8 40 | 4 81 |
| Total | 43 32 | 56 73 | 42 41 | 47 97 | 61 11 | 45 30 | 45 42 | 45 76 | 44 00 | 48 55 | 37 08 | 50 59 | 34 41 | 51 64 | 42 04 | 51 72 | 46 67 |

Mr. Ecroyd's gauge is exactly similar to that used so long by the late Dr. Dalton, and placed at Mayfield, in Manchester. The rain is received from a large funnel into a graduated vial or cylindrical receiver of smaller area than the funnel, and graduated accordingly. When the quantity is measured the receiver is emptied. Mr. Ecroyd's gauge stands about four feet from the ground; it is placed in an exposed situation in his garden, near Castle-hill, about half a mile to the south-west of Rochdale. The garden is on the top of the hill forming the southerly summit of the valley of the river Roch, probably about 500 feet above the level of the sea, and is little more than half a mile from Moss Lock on the Rochdale Canal. The correctness of the observations are said to be corroborated by those of Mr. Haworth, who keeps a similar gauge nearer Moss Lock: the variation between the two is stated to be very slight.

As bearing upon the accuracy not only of the observations of the Rochdale Canal Company, but of those instituted by the Society, these observations are very important. They show a much larger fall of rain than has been registered at Moss Lock, and go far to prove that the index rods, in rising above the tops of the gauges put down by the Society, have not materially, if at all, influenced the returns. The following table will afford a comparison:—

| | Canal Co. | | Society. | | Mr. Ecroyd. | | |
|---|---|---|---|---|---|---|---|
| | In. | Dec. | In. | Dec. | In. | Dec. | |
| 1844 .............. | 20 | 50 | 30 | 3 | 34 | 41 | |
| 1845 .............. | 29 | 43 | ... | | 51 | ~~40~~ | 64 |
| 4 1847 .............. | 26 | 82 (10 Mos.) | 31 | 3 (10 Mos.) | ~~64~~ | 58 (10 Mos.) | 46 |

It is not easy to account for the great discrepancy. It may be that Mr. Ecroyd and Mr. Haworth are more accurate observers than the lock-keeper of the Canal Company.

## II.—*On Mnemonic Aids in the Study of Analysis.* By Rev. Thomas P. Kirkman, M.A., *Rector of Croft-with-Southworth, Lancashire.*

Read February 8, 1848.

(1.) That mnemonical aids in the study of the mathematics are considered valuable, and to be worth the notice even of adepts in analysis, appears from the following article, at page 291, vol. iii., of the Cambridge *Mathematical Journal*, O. S.:—

"*Mnemonic Rule.*—The following mnemonic rule for the 'Cotangent' formula in spherical trigonometry may be found useful.

"If in any spherical triangle four parts be taken in succession, as for example A, b, C, a, consisting of two means b, C, and two extremes A, a; then, 'the product of cosines of the two means is equal to the sine of the mean *side* × cotangent of the extreme *side* — sine of the mean *angle* × cotangent of extreme *angle*.' That is,

$$\text{Cos b. cos C} = \text{sin b. cot a} - \text{sin C. cot A.}"$$

Granting fully the usefulness of this consideration, particularly to a mind familiar with mathematical analogies and symmetries, I may be allowed to entertain a doubt as to how far ordinary students of spherical trigonometry will find their memory's burden hereby lightened. But I speak the result of some experience when I say, that few youths who have capacity to comprehend the proof of this formula, will fail to retain it firmly in their memory, if it is *at first* presented to them thus:—

Cot `Ang. si Càng and cò b. co Càng are còt a. si b;

with the explanation and directions following.

The angles are to be distinguished from their opposite sides, a, b, c, not only to the eye by the letters A, B, C, but to the ear by the sounds Ang, Bang, Cang. Read now, or rather chaunt, the above mnemonic slowly, and very often, with as strong an emphasis as possible on the accented syllables; and you will thus *teach* it to your *ear*, your *tongue*, and your *lips*, which have all *their own powers* of memory. The meaning of the abbreviations si. and co. is obvious: they are put for sin. and cos., for smoothness merely. The proposition

$$\cot A. \sin C + \cos b. \cos C = \cot a. \sin b,$$

is difficult to remember, chiefly because, when you attempt to pronounce it unambiguously, it is long and inharmonious. Contract it and smooth it as above, giving it a little sing-song cadence, and the ear and other organs cheerfully undertake the task of remembering its *twelve mônosyllables*, which never can fail to suggest the expanded formula with accuracy; a task which, in some men who have more talent for language than for science, these organs will continue faithfully to discharge, after the reasoning faculty has forgotten the proof and much of the application of the theorem. But I do not see, although some persons may have sagacity enough to perceive this, that a student is the less likely to be able to prove a proposition because he can easily recall the enunciation: this may suggest, but can hardly conceal, the argument.

The mnemonical rule quoted above is a hint offered to the *judgment;* that which I have proposed is a short lesson to be *taught by rote* to the unreasoning sensuous *organs*. What these have once engraved on their tablets becomes a ready instrument of rapid thought; the judgment reads the formula without effort, and performs at leisure its proper function of interpreting and applying it.

(2.) The 12th and 13th propositions of the second book of Euclid's *Elements* are as follows:—

"In an obtuse-angled triangle, the square of the side ($b$) subtending the obtuse angle, exceeds the sum of the squares of the sides ($a$ and $c$) which contain that angle, by double the rectangle under either of these

sides, and the external segment between the obtuse angle and the perpendicular let fall from the opposite angle.

"In any triangle the square of the side ($b$) subtending an acute angle, is less than the sum of the squares of the sides ($a$ and $c$) containing that angle, by twice the rectangle under either of them, and the segment between the acute angle and the perpendicular let fall from the opposite angle."

These enunciations, to say nothing of the proofs, are something *for a beginner* to remember; nor would it be easy to express them in fewer words. Any teacher who will put a youth, even of moderate capacity, in possession of the following mnemonic, for both the proof and the properties, will be able to judge for himself of its value:—

Read bà, as well as àc, as one syllable, marking well the accent and the rhythm. Pronounce Sq.'b *squib* : SUD and DUQ are syllables, as are perc and Bang.

(3.) Draw pèrc: SUDbà is SUDsègs of 'c,
and Sq.'b is DUQàc mol sèg. op. two 'c, (A.)
as 'tùse or 'cùte is Bàng op. 'b
or Sq.'b is DUQàc le coBàng two àc. (B.)

(*a.*) Here perc signifies the *per*pendicular Cp on $c$ from C: *draw* this; then if $s$ be the segment Bp of $c$, not adjacent to $b$,

$$b^2 - a^2 = (c \pm s)^2 - s^2, \text{ or}$$
$$b^2 = a^2 + c^2 \pm s.\,2c; \qquad \text{(A).}$$

the upper or lower sign being taken as B is *obtuse* or *acute.*

(*b.*) SUD is *S*um $\times$ *D*ifference of the two quantities indicated.

SUDba is $(b + a)(b - a)$ or $b^2 - a^2$: SUDab would be $a^2 - b^2$.

SUDsegs of $c$ is Sum $\times$ Diff. of the segments Ap and Bp, or $(c \pm s)^2 - s^2$.

(*c.*) DUQ means *du*o *q*uadrata of the indicated pair of quantities;

DUQac is the two squares $a^2 + c^2$.

Sqb is squared $b$: pronounce *squib*; *squab* might mislead.

(*d.*) mol is a contraction of *mo*re or *l*ess, $\pm$

(*e.*) le is an abbreviation of *less*, or —

*seg. op.* in (A) means the segment of *c opposite*, not adjacent to, *b*, whose square is the subject of the proposition. Pronounce *two* long, to avoid confusion of it with *to*. Bang is the angle opposite to *b*.

By putting in A for *s* its value $\mp a$ cos B. we obtain

$$b^2 = a^2 + c^2 - \cos B.\ 2ac. \qquad \text{(B.)}$$

(4.) The following two groups of mnemonics give the solutions of all plane and spherical triangles. Su by D is a dissyllable; sleb and slec are syllables; s. sla is a dissyllable, as is b'c also.

Sìde to Sin òp. as Sìde to Sin òp. (*a.*)
Su by D Sìnes or sìdes is taf Sùm by taf Dìff. (*b.*)
Tàsquaf 'A is sleb slèc by s. slà, (*c.*)
the nùm, is b'c of sìsquaf 'A; (*d.*)
In sphere put sines of facs. (*e.*)
le cotàsquaf in pò. has cò's 'bove and 'lòw. (*f.*)
side 'c is co$\gamma$. sùm (*ab*) (*g.*)
where Sì$\gamma$ is chaCàng two mean àb by S (ab.)

In these two lines ab is a syllable.

These are abbreviations explained thus:—

(*a.*) *Side b* is *to* the *sine* of its *op*posite angle, *as side c to* the *sine* of its *op*posite angle, *as side a*, &c.

(*b.*) SubyD means Sum by Difference; *taf* stands for *tan*gent of hal*f*.

$$\frac{\text{sine A} + \text{sine B}}{\text{sine A} - \text{sine B}} = \frac{a+b}{a-b} = \frac{\tan.\ \frac{1}{2}\,(\text{A}+\text{B})}{\tan.\ \frac{1}{2}\,(\text{A}-\text{B})}; \qquad (b.)$$

*sines or sides* means sines of angles, or sides opposite those angles.

(*c.*) Tàsquaf is *tan squa*red of hal*f*: there is no risk of error here in pronouncing A and *a* alike;

sleb is $(s-b)$, *s* being semiperimeter; vide le (3) *e.*

$$\tan.^2 \tfrac{1}{2}\,\text{A} = \frac{(s-b)\,(s-c)}{s\,(s-a)} \qquad (c.)$$

(*d.*) The *num*erator on the right (*c*) is $bc \times \sin.^2 \frac{1}{2}$ A $= (s-b)\,(s-c)$ (*d.*)

Sisquaf is *sin squared* of hal*f*; *of* means *times* or $\times$.

(*e.*) *In spherics*, the two last formulæ (*c*) (*d*) become, *putting* the *sines of* the *factors*

$$\tan.^2 \tfrac{1}{2} A = \frac{\sin (s - b) \sin (s - c)}{\sin s \qquad \sin (s - a)} \qquad (ec)$$

$$\sin.^2 \tfrac{1}{2} A = \frac{\sin (s - b) \sin (s - c)}{\sin b \qquad \sin c} \cdot$$

(*f.*) *In* the supplemental *pol*ar formula made from (*ec*), we have le cotasquaf (*le*ss *cotasqua*red of hal*f*) on the left, and *cosines* ab*ove* and be*low* on the right: or,

$$- \text{Cot.}^2 \tfrac{1}{2} a = \frac{\text{Cos (S—B). Cos (S—C)}}{\text{Cos S. Cos (S—A.)}}$$

(*g.*) $c = \text{Cos}\ \gamma\ (a + b)$;

Where sine $\gamma = \text{Cos} \tfrac{1}{2}\text{C}. \dfrac{2 \quad (a.b)^{\frac{1}{2}}.}{a + b.}$

Sum ab is $a + b$; sometimes Sab.

*Si*$\gamma$ is sine of $\gamma$. pron. sig. *Cha* is *c*os of *ha*lf; pron. as in chance; *mean ab* is the mean proportional between $a$ and $b$, or $(ab)^{\frac{1}{2}}$.

(5.) Sìng to sin òp as Sìng to sinòp. (*h*)
Circ pàrts are ă 'b and sup. rèst, and
Smìd is pro. Còps or pro. Tàds. (*i*)
Tàf Sor Dàngs by Còth Càng
is CòrShaDàb by CòrShàM; (*k*)
The dèn has tàf in pōle. (*l*)
Cò$\varphi$. còc. is Cà. co. $\varphi$lèb,
(prim. ùlt. are sìnes in pòle.)
Where tàn $\varphi$ is tàna. co Cang; (*m*)
(for tàn $\varphi$ say còt $\varphi$ in pòle.)

(ă 'b in the second line is a dissyllable, CòrShaDàb a trissyllable, $\varphi$leb a syllable, as also tan $\varphi$, cot $\varphi$.)

(*h.*) *Sing* is *si*ne of a*ng*le: sin A : sin $a$ = sin B : sin $b$ = sin C : sin $c$.

(*i.*) Neper's circular parts are $a$, $b$, and *sup*plements of the *rest*, or of $c$ AB; and *s*ine *mid*dle part is *pro*duct of *C*osines of *op*posite*s*, or *pro*duct of *T*angents of *ad*jacent*s*.

(*k.*) *taf*, vid (*b*). *SorD* is sum or diff. *Coth* is *cot*ang of *h*alf. CorSha is *c*os *or* *s*in of *ha*lf. Dab is difference of $a$ and $b$. M stands here, and in many other mnemonics, conveniently for suM ; and refers to the quantities $a$ and $b$ in Dab. In CorS in the second, and SorD in the first line, the cos goes with sum, and sine with difference: no ambiguity can arise from the two meanings of S in CorS and SorD. We have here the two formulæ:

$$\frac{\tan \frac{1}{2}(A \pm B)}{\cot \frac{1}{2} C} = \frac{\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}(a-b)}{\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}(a+b)}$$ : or, read at

length, (*k*) is the tan. of half sum (or diff.) of angles AB, divided by the cotan. of half C, is the cosine (or sine) of half the difference of the sides $ab$, divided by the cosine (or sine) of half their sum.

(*l.*) The *deno*minator on the left of (*k*) has *taf* instead of *coth*, in the supplemental *pol*ar formula; or,

$$\frac{\tan \frac{1}{2}(a \pm b)}{\tan \frac{1}{2} c} = \frac{\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}(A-B)}{\begin{smallmatrix}\cos\\ \sin\end{smallmatrix} \frac{1}{2}(A+B)}$$

(*m.*) $\cos \varphi . \cos c = \cos a . \cos (\varphi - b)$, where $\tan \varphi = \tan a . \cos C$: and, in the supplemental polar formula,

$\sin \varphi \cos C = \cos A, \sin (\varphi - B)$, where $\cot \varphi = \tan A \cos c$; i e., the first and last (*prim. ult*) are sines instead of cosines, and $\tan \varphi$ becomes $\cot \varphi$.

From (*b*)(*k*)we find A and B in terms of $a$ $b$ $C$; from (*c*) (*ec*) we obtain A in terms of $a$ $b$ $c$ ; *f* gives $a$ in terms of A B C ; (*l*) gives $a$ and $b$ in terms of A B $c$; (*g*) and (*m*) give $c$ in terms of $a$ $b$ C, and (*m*) gives also C in terms of A B $c$.

(6.) If space permitted me, it would be easy to give dozens of such mnemonics in trigonometry. The following will be intelligible, with the remark that *chash* is *c*os *ha*lf *s*in *h*alf, referring to SUD and to *two*:

CórS is SùD or twò chàsh. (*o*) (*v.* 3, *b*)
SìM mol SìD's two SórC. CòrS. (*p*) (*v.* 5, *k*)
CòD mol CòM's two CòrS. CòrS. (*q*)

The antecedents of the two *ors* in any line are taken together, as are the consequents.

(*o*.) Cos $\theta = \cos^2 \frac{1}{2}\theta - \sin^2 \frac{1}{2}\theta$; sin $\theta = 2 \cos \frac{1}{2}\theta \sin \frac{1}{2}\theta$.

(*p*.) Sin $(a + b) + \sin (a - b) = 2 \sin a \cos b$; sin $(a + b) - \sin (a - b) = 2 \cos a \sin b$.

(*q*.) Cos $(a - b) + \cos (a + b) = 2 \cos a \cos b$; cos $(a - b) - \cos (a + b) = 2 \sin a \sin b$.

Sin of sum more (or less) sine of diff. (*p*) is 2 sin (or cos) × cos. (or sine), &c.

(7.) The following trigonometrical formulæ are important in integrations:

For '2^ton^ Co^to^ Cos *o*'$\theta$ will gò (*a*)
2'$\theta$ less hàlf way wìth o bìn.
Sin. ^tèv.orod.ò^ just lìke Co^to.^ (*b*)
but CorS. *o*$\theta$ fòr Cos ò$\theta$ repìn. (*c*)

(*a*.) 2^ton^, (pron. twoton) is $2^n$, or 2 to (power) *n*. Co^to^ is cosine to (power) *o*, or $\text{Cos}^{n+1}$, *o* standing here for the next higher number than *n*, as being the next letter in order alphabetical: $\theta$ is understood after Co.

For the expansion of $2^n \text{Cos}^{n+1}\theta$, Cos $(n + 1)\theta$ (*Cos o*$\theta$) *will go* 2$\theta$ *less* (pron. toothless), i.e.,

(*a*.) $2^n \text{Cos}\,(n + 1)\,\theta = \text{Cos}\,(n + 1)\,\theta + \text{A Cos}\,(n - 1)\,\theta + \text{B Cos}\,(n - 3)\,\theta + \ldots$ this *goes with o bin;* i.e., the co-efficients 1 A B C are the terms of $(1 + 1)^{n+1}$, the $(n + 1)$ th binomial, or *o bin* series: and this *goes* just *half way,* for if $(1+1)^{n+1}$ has an even number of terms, the expansion has exactly half of them; and just half also if $(1 + 1)^{n+1}$ has an odd number, for the last term of the expansion is then multiplied by half the middle term of the binomial series.

(*b*.) Sin^tèvorodò^ (five syllables) is the same expansion with Co^to^, except that *Sin to even o* or $\text{Sin}^{n+1}\,\theta$, $(n + 1)$ being even, has its terms Cos $(n + 1)\,\theta$, A Cos $(n - 1)\,\theta$, &c., written *repin:* and *Sin to odd o*, $\text{Sin}^{n+1}\theta$ when $n + 1$ is odd, has the series Sin $(n + 1)\,\theta$, A Sin $(n - 1)\,\theta$, B Sin $(n - 3)\,\theta$, &c., written *repin.*

(*c*.) CorS o $\theta$ is (Cos or Sin) $(n + 1)\,\theta$, as the index $n + 1$ is even or odd, the ors in (*b*) and (*c*) corresponding as to antecedents and consequents.

*pin* is an abbreviation of *p*lus m*in*us, $+ - + - \ldots\ldots$

*repin* is plus and minus alternately, *re*versed;

or $\ldots\ldots\ldots + - + - +$ read backwards from the last term.

Examples are

$$2^4 \operatorname{Cos}^5\theta = \operatorname{Cos} 5\theta + 5 \operatorname{Cos} 3\theta + 10 \operatorname{Cos} \theta$$

$$2^5 \operatorname{Cos}^6\theta = \operatorname{Cos} 6\theta + 6 \operatorname{Cos} 4\theta + 15 \operatorname{Cos} 2\theta + 10$$

$$2^4 \operatorname{Sin}^5\theta = \operatorname{Sin} 5\theta - 5 \operatorname{Sin} 3\theta + 10 \operatorname{Sin} \theta$$

$$2^5 \operatorname{Sin}^6\theta = - \operatorname{Cos} 6\theta + 6 \operatorname{Cos} 4\theta - 15 \operatorname{Cos} 2\theta + 10$$

(8.) I shall now proceed to select a few mnemonics, from the multitude which I have been accustomed to employ, so as to exhibit instances of various mnemonical devices, and shall pay no regard to the order of the mathematical topics to which they belong. None of these devices have been invented for one emergency only, being all of them repeatedly applicable in the concise expression of formulæ.

(*a.*) per *ab* is $(a^2 - b^2)^{\frac{1}{2}}$, per *ba* is $(b^2 - a^2)^{\frac{1}{2}}$;

(*b.*) poth *ab* is $(a^2 + b^2)^{\frac{1}{2}}$.

$(a^2 - b^2)^{\frac{1}{2}}$ is the *per*pendicular in the right angle triangle whose base is $b$, and whose hypothenuse is $a$: and $(a^2 + b^2)^{\frac{1}{2}}$ is hy*poth*enuse to the sides $a$ and $b$ about the right angle.

Thus, to remember the equation to the conchoid, say,—

(*xy̆* a dissyllable,)

In conch, *a* pole ord, and *f* mŏd,

*xy̆* is mol pĕr *fy* Sum *yă*.

*In* the *conch*oid, *a* being the *ord*inate of the *pole*, *and f* the *mod*ulus,

$$xy = \pm (f^2 - y^2)^{\frac{1}{2}}.(y + a).$$

Again,

Wĭtch *x'y̆* is twŏ *a* per *ă* D*ax*.

In the witch, $xy = 2a\,(a^2 - (a - x)^2)^{\frac{1}{2}} = 2a\,(2ax - x^2)^{\frac{1}{2}}$

D*ax* being $a - x$, per $a$ D*ax* is $(a^2 - (a - x)^2)^{\frac{1}{2}}$: D means Diff. here.

(*c.*) If $m = ax$ and $n = by$, $(a^2x^2 - b^2y^2)^{\frac{1}{2}} = (m^2 - n^2)^{\frac{1}{2}}$ is a perpendicular of the same kind: call this *perprod* (*ax by*), or smoother, *peprod* (*ax by*), or shorter still, *pep.* (*ax by*): i. e., the

perpendicular in the triangle whose base is the product $by$, and hypothenuse the product $ax$. The equation of the lemniscate is, ($\bar{y}x$ one syllable,)

DUQ$\bar{y}x$ in Lemnì is pèprod ($ax$ $b\grave{y}$),

and pèp (co$\theta$ $a$ $'bsi\theta$) is $r$; i. e.,

$$y^2 + x^2 = (a^2x^2 - b^2y^2)^{\frac{1}{2}}, \qquad \text{or,}$$

$$(\cos.^2\theta.a^2 - b^2\sin.^2\theta)^{\frac{1}{2}} = r.$$

for DŬQ vide 3-c.

($d$.) If $ax = k^2$ and $by = l^2$, $(ax - by)^{\frac{1}{2}}$ is $(k^2 - l^2)^{\frac{1}{2}}$; or the perpendicular to base $(by)^{\frac{1}{2}}$ and hypothenuse $(ax)^{\frac{1}{2}}$, both given mean proportionals: call this *per mean* ($ax$ $by$), or *perean* ($ax$ $by$).

($e$.) Bi Cáng is pèrean ($\grave{a}b$ *segs*),

($f$.) or Còsha Càng of Hàrm. legs,

($g$.) and *vì*. $ab$ is the *vì*. *segs*.

i. e., if $d$ be the BiCang, or *bi*sector of $C$, the vertical angle of a triangle, $s$ and $s_1$ being the two segments of $c$ made by it,

$$d = (ab - ss_1)^{\frac{1}{2}} \qquad (e)$$

$$d = \text{Cos } \tfrac{1}{2}\, C \times \frac{2\,ab}{a+b} \qquad (f)$$

$$\frac{a}{b} = \frac{s}{s_1} \qquad (g)$$

($f$.) Cosha in ($f$) is *Co*sine of *ha*lf: *of* is $\times$, when between two *quantities*.

*Harm.* is *har*monic *mean*, of the legs $a$ and $b$;

Harm $\grave{a}b$ is two $\grave{a}b$ by $S\grave{a}b$.

($g$.) *vi* in $g$ is a contraction for *quote of*, (di*v*ide); the quote of $a$ and $b$ is the quote of the segments of the base $c$.

For an example of the use of *poth*, take one from Alg. Geom.

($h$.) lòr's the còn. by pòth fìcs;

with which may be given the corresponding

($i$.) plòr's the còn. by dìg fìcs.

In the line $ax + by = c$, referred to right axes, if we call *lor* the shortest distance $p$ of the *l*ine from the *or*igin,

$$p = \frac{c}{(a^2 + b^2)^{\frac{1}{2}}} \qquad (h), \qquad \text{i. e.,}$$

lor is the *con*stant $c$ divided *by* the hy*pothe*nuse to the *fics* or coef*fi*cient*s*, $a$ and $b$, of the variables.

In ($i$), *plor* is the shortest distance $P$ from the *or*igin, of the *pl*ane $Ax + By + Cz = H$, referred to right axes.

*dig* means *diag*onal, sometimes *diag* or *dia.* Dig *bac* is $(a^2 + b^2 + c^2)^{\frac{1}{2}}$.

*dig fics* is $(A^2 + B^2 + C^2)^{\frac{1}{2}}$, diagonal of the right solid whose sides are A, B, and C. Then we have ($i$) thus explained,

$$P = \frac{H}{(A^2 + B^2 + C^2)^{\frac{1}{2}}}$$

(9.) Useful formulæ are the following on lines and planes referred to right axes.

plìn's vagilȣ̀, if lòr's con in ȣ̀; ($a$)
plàp is vaplȣ̀, if plòr's con in ȣ̀. ($b$)

($a$.) *plin* is the distance of a point $p$ from a *line*

$$ay + bx + c = 0\,;$$

*gil* means *gi*ven *l*ine ; *gil* ȣ is the ȣδεν or zero just written; *vagil* ȣ is the *va*lue of that expression, when $x_1y_1$ of the point $p$ are put for $xy$, or $ay_1 + bx_1 + c$. This is the length *plin*, if the *con*stant $c$ *in* that ȣδεν is the length *lor*, (8, $h$).

($b$.) *plap* is the distance of a point $p$ from a plane

$$Ax + By + Cz + H = 0.$$

*vaplȣ* is the *va*lue of this *pl*ȣ, or plane's ȣδεν, at $p$ $(x_1y_1z_1)$ : and this value, $Ax_1 + By_1 + Cz_1 + H$, is the length *plap*, provided that the *con*stant H in that ȣ or zero be the length *plor*, (8,$i$,).

Generally, let the co-ordinate axes contain an angle V: Plin by sine V' is vĕgìl ȣ at '$p$ by 'bas (fics V').

($c$) *Plin* and vagil ȣ as above.

($c$.) *bas fies V* is the *bas*e of the triangle, whose sides are the *fics*, or coef*fi*cient*s* of the variables $x$ and $y$, and contained angle V. That is,

$$\frac{\text{Plin}}{\text{Sin. V}} = \frac{(ax_1 + bx_1 + c)}{(a^2 - 2ab\ \text{Cos.V} + b^2)^{\frac{1}{2}}} \qquad (c)$$

and this is true whatever $c$ may be, whether *lor* or not.

If my space were not limited, I could easily give mnemonics, equally brief and simple, for all the formulæ required about the

inclinations and intersections of lines and planes, for both right and oblique axes.

(10.) The properties of the conic sections are easily remembered, when the student has learned to *talk to himself* about them in rapid and unambiguous syllables like the following. The equation to the central conics being—

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1,$$

the following mnemonics are a few examples out of many.

[Pron. (1 è), un è, a dissyllable.]

Sqỳ is SUD (1 è) SUD (à x) ; ▾ (3*b*) (*a*)

2 squìb's larec à ; per (àb) is eà ; ▾ (3 c) (8 a) (*b*)

at or' foc the x' is èa mol x', ▾ (3, d) (*c*)

òrfoc, r's làrec by ('2 mo 2 ècoθ) : (*d*)

à mol èx or èx mol à ▾ (3, *d*) (*e*)

are fòps in 'li'pse or hỳ'bola :

dìct is bà by mèan fops : ▾ (4, *g*) (*f*)

dìft is b' of roòq fops, ▾ (8, *f*) (*g*)

and b' is meàn of dìfts ; (*h*)

bà by dìct is cònja : (*i*)

mèan of fòps is cònja : (*k*)

rhò. ba is cù conjà ; (*l*)

rhò ba is cùper (a èx) : ▾ (8, *a*) (*m*)

pèr (poc 'b) is xè. x e a dissyl. ▾ (8, *a*) (*n*)

(*a.*) $y^2=(1-e^2)(a^2-x^2)$ ▾ SUD, (3 *b*)

(*b.*) $2b^2=la$, *l* being *larec*, or *lat*us *rect*um : $(a^2-b^2)^{\frac{1}{2}}=ea$.

(*c.*) The *or*igin is *at* the *foc*us, when the equation is—

$$\frac{(ea\pm x)^2}{a^2}+\frac{y^2}{b^2}=1 \qquad \text{vid mol. (3, } d$$

(*d.*) or $r=\dfrac{l}{2\mp 2e\cos.\theta}$ mo is + under a vinculum.

(*e.*) $(a\pm ex)$ are the distances from *fo*ci of a point *p*, or the *fops*, in the *ellipse*, and $(ex\pm a)$ the *fops* in the hyperbola.

(*f.*) *dict* is *di*stance of *c*entre from *t*angent, and $=$ *ba* : $(a^2-e^2x^2)^{\frac{1}{2}}$, or *ba* by *mean* of *fops*, $=$ *ba* : $(ff')^{\frac{1}{2}}$, if $f$ and $f'$ are fops.

(*g.*) ***dift*** is *di*stance of *f*ocus from *t*angent, and $= b.(f:f')^{\frac{1}{2}}$ or *b of root* of *quote* of fops. v. (8, *f*) ***rooq*** is square ***root*** of *quote*.

(*h.*) $b$ is the mean proportional of the two difts.

(*i.*) conja is the semi-diameter *á conj*ugate to a point $(x_1, y_1)$ $a' = ba$ by dict; $a' = (ff')^{\frac{1}{2}}$. (*h.*)

(*l.*) ***rho*** is $\varrho$, the radius of curvature; $\varrho\, ba = a'^3$; *cu*bed $a'$.

(*m.*) $\varrho\, ba = (a^2 - e^2x^2)^{\frac{3}{2}}$, a *cu*bed *per*pendicular, (8, *a.*)

(*n.*) ***poc*** is distance of $(xy)$ or $p$ from *c*entre, $= \mathrm{R}$: $(\mathrm{R}^2 - b^2)^{\frac{1}{2}} = ex$.

This last property (compare *e*) is expressed by Leslie in his "Geometrical Analysis," in the following luminous and encouraging language; Prop. vii. p. 206:—

"If the transverse axis of an ellipse or hyperbola be divided into segments equal to lines drawn from the foci to any point in the curve, the square of its distance from the centre will be equivalent to the sum or difference of the squares of the semi-conjugate axis, and the distance of intermediate section from the centre."

Alas for the student who is doomed to pick up his notions of advanced geometry from such authors as Leslie!

It is understood, of course, in all that precedes in this article, that $b^2$ is, of either sign, positive for the ellipse, and negative for the hyperbola.

(11.) Many persons find it difficult to remember the principal theorems in combinations. I found it so, for one, until I taught them to my ear and tongue in the fashion following—

If 'p ele'ms. are 'm, a's; è, b's; ì, c's;
The pe'rms in p are 'p fags (*a*)
by 'm fags. è fags. ì fags.
The còmb. repè's of 'n in 'p's
are 'p-n-ùps by 'p fags. (*b*)
Com. nòn-repè's of 'n in 'p's
are 'p-n-bàcks by 'p fags. (*c*)
Non rèpe. vars. of 'n in 'p's are 'p-n-backs; (*d*)
The rèpe. vars. of 'n in 'p's, are 'n to p[th]. (*e*)

(*a.*) If there be $p$ symbols or *elements*, viz., m, a's ; e, b's ; i, c's • • • • the number of permutations p together is

$$\frac{1\cdot2\cdot3\cdots\overline{p-1}\cdot p}{1\cdot2\cdots m\cdot1\cdot2\cdots e\cdot1\cdot2\cdots i\cdots\cdot}$$

*fags* is *fa*ctor di*g*its. *Five fags* is $1\cdot2\cdot3\cdot4\cdot5$.

(*b.*) The *repe*ating *com*binations (of $n$ in $p$'s,) say of 7 in 4's, are $\frac{7\cdot8\cdot9\cdot10}{1\cdot2\cdot3\cdot4}$, or four-seven-ups by four fags.

*Three-2-ups* are $2\cdot3\cdot4$.

By *repe*ating *com*binations are meant such as *may* contain a letter or letters repeated, as 7662, 7744, 7111, 7654, 7777.

(*c.*) *Com*binations *non repe*ating (of $n$ in $p$'s,) say of 7 in fours, such as 7654, &c., are in number

$$\frac{7\cdot6\cdot5\cdot4}{1\cdot2\cdot3\cdot4},$$ four-7-backs by four fags.

Two three backs are $3\cdot2$.

(*d.*) *Non-repe*ating *var*iations are the non-repeating combinations permuted : thus the repe. vars. of 7 in 4's will include 7654, 7645, 7564, &c., being in number $7\cdot6\cdot5\cdot4$.

(*e.*) The *repe*ating *var*iations of 7 in 4's include 7777, 7767, 7776, 6777, &c., and are in number $7^4$ : $n$ to $p^{\text{th}}$ power.

(12.) In the theory of numbers most students find difficulty in retaining both the results and the demonstrations. Here follows an example of a mnemonic to aid in remembering both: Make dissyllables of em, etol, etom ;

If mùps are ĕ'm and 'm is prì
vi ém has the rèm. of ètol vì ;
(A.) You pùt eto'm Suto'm (un 'd)
The twò extrè. will hàve the rè ;
Then pùt dtom Suto'm (un 'c) ; &c.

1 (òver 'g) is g·ìnt
(B.) if è$^{tog}$ ìs e·m·ìnt ;
and è if there ìs no 'g
a prìm root of 'm will be.

(A.) If e and m are *mups*, *mu*tually *p*rimes, and m is prime, *viem*, (8, g) or e : m, has the *rem*ainder of $e^l$ *vi*, or $e^{m-1}$ (e : m), or $e^m : m$; l is m — 1, as in (7, a). To prove this, *you put* $e^m = (1 + d)^m$, *Sum to* power *m*; *un* is unity: *d* is e — 1, (7, a): *then put* $d^m = (1 + c)^m$, or $(1 + (e - 2))^m$, c being d — 1, (7, a). The remainder of $e^m : m$ is thus seen to be that of $\{1 + (e - 1)^m\} : m$, also that of $\{2 + (e - 2)^m\} : m$, &c. *The two extre*me terms of the expansion *will have the re*mainder.

(B.) If g, being less than m — 1, (l *over* g), is such that $e^g$ *is em* × *int*eger, or $e^{g-1}$ is divisible by m, then *l is g* × *int*, or m — 1 is divisible by g; and *if there is no* such number as *g*, e is one of the *prim*itive *roots of* m. *Vide* Murphy's "Equations," § 51.

Let not the rhymes and scanning provoke a smile. There is some science, mnemonic and mathematical, in the rhyme; and the ear is ever grateful for the humblest jingle. It is a hundred to one that the reader is continually indebted, to this day, to an old and not very neat mnemonic, "Thirty days hath September," memorable for its rhyme:

> "Except in leap year, then's the time,
> February's days are twenty and nine.'

(13.) As examples of series and expansions, the following may be introduced; and first the binomial theorem

(A.) Sutôn (un 'r)? — write frω on r; (pron. Frow.)
Then r to i you multiply
with {i-n-backs by i fags.} vid. (11, c, a)

(B.) If 'n has den è, put 'r vi (rè); num dìts wed è.

(A.) Suton un r is *sum to* power *n* of (*un*ity and *r*.) What is the expansion of $(1 + r)^n$? Write frω on r; i. e.,

$$r^0 + \quad r^1 + \quad r^2 + \; . \; . \; .$$

*on* means with rising indices; b on is $b + b^2 + b^3 + \cdots$; b *fr on*, is $b^0 + b^1 + b^2 + \cdots$, or b *from* $\underline{\text{ωδεν}}$ power *on*, b on from zero power. *Then r to i* or $r^i$ *you multiply* with (or by $(n \cdot \overline{n-1} \cdot \overline{n-2} \cdots \overline{n-i-1}) : 1 \cdot 2 \cdot 3 \cdots i$.

(B.) If $n$ is a fraction *having* e for *den*ominator, say n : e, you *put* r : e (*vi re*) for $r$ : writing out $(r:e)^0 + (r:e)^1 + (r:e)^2$ &c. : *num dits wed e*; i. e., the *num*erator *digits* are affected by the factor e; *wed* means, are multiplied by: so that the co-efficient of $(r:e)^i$ is $\{n \cdot \overline{n-e} \cdot \overline{n-2e} \cdots (n-(i-1)\,e)\} : 1 \cdot 2 \cdot 3 \cdots i \cdot$ And è may be of either sign. The tyro who has this mnemonic at his tongue's end, and understands it, will see no difficulty in the expansion of *any* binomial $a^m\{1+(b:a)\}^m = (a+b)^m$.

We say multiply *with* in (A); *by* should always denote division.

(C.) gòlt is frᵘ tòng.

$$\text{This means } \varepsilon^t = t^0 + \frac{t^1}{1} + \frac{t^2}{1 \cdot 2} + \frac{t^3}{1 \cdot 2 \cdot 3} + \&c.$$

I call $\varepsilon^t$, or $\log^{-1} t$, golt; $\varepsilon^{ax}$ is gol(ax); gol is log read backwards: *frᵘ t on* vid (A) above. *t ong* differs from *t on*, in that every power is divided in the former by the *fags* of that power: the zero power is unity in both of course. There is no ambiguity in pronouncing t ong here as one syllable *tong*.

(D.) ăᵗᵒʸ le òne 's vi òng ($\breve{y}$ mòd) vid. C, vid. (3,*e*) (8,*g*.)

and mòd is cì logà: i.e.

$$a^y - 1 = \frac{y:m}{1} + \frac{(y:m)^2}{1 \cdot 2} + \frac{(y:m)^3}{1 \cdot 2 \cdot 3 \cdot} + \cdots :$$

this is *vi ong*, or a quote *ong*; the quote of $y$ and $m$, $m$ being modulus of the system: and the *mod.* m *is* $(\log.a)^{-1}$; the reciprocal of hyp. *log.a*.

(E.) If ù be logà

vi ($\Delta$u mod)'s pin incà.ᵗʰˢ on by—dex. i. e.,

If $u = \log a$, and $h$ be the *inc*rement of $a$; and $u_1 =$ $\log(a+h)$;

$$\frac{\Delta u}{mod} = \frac{u_1 - u}{mod} = \frac{h}{a} - \frac{h^2}{2a^2} + \frac{h^3}{3a^3} - \frac{h^4}{4a^4} \cdots\cdots$$

pin vid. (7 *c*). incaths is the fraction $\frac{inc}{a} = \frac{h}{a}$

vi ($\Delta$u mod) (8, *g*), is this fraction *pin on by index* of the power.

(F.) Nᵗʰ vàl is n-ˢbino fun oòp.

(G.) $\Delta$tor *u* 's *r*-bìn fun ùp repìn; (pron. Deltòroo.)

(F.) If $u_0\, u_1\, u_2\, u_3$ be any function and its successive values, fun up is $u_0 + u_1 + u_2 + u_3 + \ldots$ or *fun*ction up: *up* means with increasing subindices.

fun oop is $u + \Delta u + \Delta^2 u + \Delta^3 u + \ldots$

*oop* means + the successive differences: vid. *n-bino*, *r-bin*, (7,*a*).

n-bino fun up is fun up with the co-efficients $(1 + 1)^n$.

$$\text{(F.)} \quad u_n = u + n \Delta u + \frac{n.\overline{n-1}}{1.2} \Delta^2 u + \&c.$$

$$\text{(G.)} \quad \Delta^r u = \pm u \mp r.u_1 \pm \frac{r.\overline{r-1}}{1.2} u_2 \mp \frac{r.\overline{r-1}.\overline{r-2}}{1.2.3} u_3 \pm \ldots$$

the upper signs to be taken when $r$ is even, the lower when $r$ is odd, so that the series is *repin*, (7, *c*), the final term being always positive.

(H.) ù.inc fid 't-adds, or 'le reci 'v-adds

($t = u - 1,\ v = u + 1$; *v.* (7,*a*) (3,*e*) )

is ù-or cip ù-adds ad fìn. or ab ìn.

(*reci* = *cip* = *ci*, *v.*D).

(The three *ors* correspond; *i. e.* the three antecedents go together, and the three consequents.)

This is the rule for summing factorials or differencing. Let $p\, p_1 p_2 p_3 \ldots\ldots p_u$ be an arithmetical series whose *inc*rement or common difference is $i$,

$$ui\Sigma.\, p_1 p_2 p_3 \ldots p_{u-1} = p\, p_1 p_2 \ldots p_{u-1} + C$$

$$ui\Sigma.\, \frac{-1}{p\, p_1 p_2 \ldots p_u} = \frac{1}{p\, p_1 p_2 \ldots p_{u-1}} + C$$

The symbol $\Sigma$ which is $\Delta^{-1}$, or reversed *dif*, I call *fid*: I call the factors *adds*; being terms made by *add*itions of the *inc*rement $i$. The first of the above equations has $u$-1 *adds* on the left, and $u$ *adds* on the right, terminating with the final one (*ad. fin.*) of those on the left: the second has $u + 1$ *adds* on the left, and $u$ *adds* on the right, beginning from the first one on the left (*ab in*itio). The constant is understood; and this mnemonic for summation serves of course for differencing, if $\Delta^{-1}$ be transferred as $\Delta$ to the other side.

Want of space compels me to suppress all examples of mnemonics on the Differential and Integral Calculus, on Equations, on

Mechanics, on Natural Philosophy, and on the higher Mathematics, although the power of the devices to which I have resorted for the study and retention of these subjects, is equally advantageous in the leading topics of them all. Nor do I regret the omission of what I might further have to say. The example of Dr. Richard Grey, rector of Hinton, in Northamptonshire, shows how little honour, either from students or masters, awaits the inventor of mnemonical aids. Great as is the value of the "Memoria Technica," unrivalled for the simplicity and power of its devices, it is yet, after the lapse of more than a century, but little known, and less admired. Ask the first M.A. or D.D. that you meet with, what day of the month Cesar's augur exactly meant by the ides of March, and it is a hundred to one that you will be none the wiser. He will tell you of some little book in which you will meet with the information; but he will have long forgotten the Roman Calendar. If any person points out to him the single mnemonical hexameter into which the philosophical Richard Grey has compressed both the Roman and the modern calendars, he will forthwith find it a laughable conceit of the learned Doctor, that the remembrance of jargon such as that can fail to be incomparably more difficult than the retention of the matter to be suggested thereby! He must be either a very great, or a very little man, who can indulge the hope, that any suggestion of his, for the improvement of established methods of tuition, will be favourably received, or confessed to have any value.

## III.—*On the Formation of Dew.* By Thomas Hopkins, Esq.

Read February 22, 1848.

The nature of Dew and the mode of its formation have long engaged the attention of enquirers, and many speculations and opinions have been advanced respecting it. It is common to speak of the *rising* of the dew—some parties maintaining that it rises from the earth. Others have contended that it falls from the sky; and this latter view is countenanced by the common way of speaking of *falling dew*. A paragraph lately appeared in the public newspapers, stating that "a French savant has recently published two letters to prove that dew does not arise from the earth, or fall from the sky, but is formed by the elastic and invisible vapour diffused throughout space, which surrounds bodies."

The labours of Dr. Dalton, Wells, and others, have thrown much light on the nature of dew; but the attention recently bestowed on meteorology, and the large mass of facts accumulated relating to it, may possibly enable us to obtain a more full view of the phenomena attending the formation of dew than had been previously presented.

Those who were of opinion that dew *rose* from the earth, did not maintain that it came thence in the form of globules of water, as it is seen by us, but that the aeriform material of which it is constituted was supplied directly from the earth. And those who asserted that the dew *fell*, assumed that it was formed from vapour at some height in the atmosphere, whence it descended to the surface of the earth. Thus the idea, that dew is formed from "the elastic and invisible

vapour" of the atmosphere, advanced by the French savant, is an old one—the different opinions which then existed having reference merely to the manner in which the liquid dew was formed from the vapour.

In this as in many other cases disputes appear to have arisen from the same word having been used to express different ideas. The word *dew* is sometimes used to express the drops of water on the leaves of grass, as, in speaking of the "dew on the grass," meaning the drops of water that under certain circumstances are found on grass. At other times it is meant to convey the idea we have of the small globules of water that float in the air near to the ground, and then the word is synonymous with "low mist;" whilst it is occasionally spoken of as the aeriform material from which both the drops and globules are formed, and is then used to designate the aqueous vapour itself.

Dew and mist are formed from the aqueous vapour that exists in the atmosphere, by a degree of cold that is sufficient to produce condensation of a part of the vapour. The two names designate, not different substances, but the same substance produced in different ways. Dew has, therefore, to be distinguished from mist only by the mode and place of its formation, and the shape in which it exists.

Heat is found to leave all substances by radiation. In the middle of the day, under ordinary circumstances, the radiant heat received from the sun more than counterbalances the loss of heat radiated from the earth; but as the radiated solar heat diminishes on the approach of night, terrestrial radiation continues, and reduces the earth's temperature. And when the temperature of the earth and the air that is near to it are thus reduced below the point of condensation, or the *dew-point*, a part of the aqueous vapour of the atmosphere close to the surface of the earth is condensed, and forms particles of water so minute as to be sustained by the atmosphere, and may therefore be called mist or "*floating*

*dew;*" as, in the absence of the sun, the cooling influence of terrestrial radiation increases, more vapour is condensed, and the condensation takes place at a greater distance from the earth's surface. This extension of condensation may seem to an observer to be a rising of the dew, because it will appear successively at greater distances from the surface: yet the floating globules may not really have risen, but, from the operation of the causes just pointed out, may have been all formed in the part in which they are seen, the apparent rising of the dew being an optical deception. At other times, however, floating dew may rise. Wherever aqueous vapour is condensed into water, heat is liberated, and this liberated heat may possibly expand the air in the locality sufficiently to cause it to ascend and carry the floating dew with it to a greater height. In this way the dew may really rise, if a sufficient amount of heat be liberated. When the dew extends to a moderate height it is generally called *mist*, and may frequently be seen filling our valleys, and at a distance looking like water.

What is called "falling dew" may be often felt in the evenings in this part of the world. But it is very common in the latter end of summer and in autumn in calm weather, and appears to be produced in the following way. During the hotter part of the day, and until about four o'clock in the afternoon, the heat of the sun raises vapour to the higher part of the atmosphere, and forms cumuli or day-clouds. Soon after this time these day-clouds begin to evaporate, and consequently to cool the atmosphere in the part. The portion of the atmosphere thus cooled is thereby made heavier than adjoining portions at the same height not similarly affected—the previous equilibrium of atmospheric pressure is then destroyed, and the cooled part sinks to a lower level. As evaporation of the cloud proceeds greater cold is produced, and by the time that the whole of the cloud is evaporated, the mass of air is so much cooled as frequently

to become heavy enough to sink to the surface of the earth, where it constitutes the cold air that is often felt in the evenings succeeding warm days in the summer and autumn; —in some places known as the *land breeze.* When the day cloud is very large, the atmospheric mass is sometimes sufficiently cooled to cause it to descend to the surface of the earth, before the globules of the water constituting the cloud are all evaporated. These globules are then found floating in the lower air, and any object passing through them is soon wetted by them as if by rain, though they do not, like drops of rain, fall freely to the ground. In Lancashire, in the month of September, the clouds raised daily from the Irish Sea frequently descend in the evening to the earth, and they are abundant over the lower levels, particularly on the river Irwell, where they are well known to boatmen under the common appellation of "falling dew," and are remarkable for their intense cold, the effect of previous evaporation of a portion of the cloud in the higher part of the atmosphere. As these masses of floating particles of water are formed in the higher part of the atmosphere, they are in their origin clouds, though called falling dew when they reach a low level. It is obvious that either the floating dew formed near the surface, or this "falling dew," if carried along by a light breeze, will impinge upon, and attach to, any projecting object. In like manner, persons passing through a mass of this nature come in contact with the globules and are wetted by them, as is well known to coachmen, boatmen, and other persons similarly exposed.

But dew also forms on objects by a process differing from those just named, and this dew is found attached to different substances in very unequal quantities. The daily clouds are often evaporated early in the evening, and the sky left clear, yet highly charged with transparent aqueous vapour. At such times, radiation of heat cools the surface of the earth until its temperature sinks below the dew-point of the

atmosphere, when that part of the vapour which is in contact with the earth is condensed into water, and becomes *liquid dew*. As the earth is successively cooled by radiation below the dew-point of the air, more dew is deposited, until in this way a considerable part of the vapour of the lower portion of the atmosphere may be abstracted from it, and collected on the surface in the form of water.

A French chemist, C. A. Prieur, has maintained, "that the moisture deposited on bodies soon after sunset, is not the same with that we find on them again at sunrise. There is consequently (he says) an interruption in the phenomenon—an evaporation of the *serein* or evening dew, and a new production in the morning," *rosée*.

It has been shown that what is called evening dew is often descended cloud, and it is commonly found only for a short time after the sun has set, as it soon evaporates or is deposited. The cold, too, produced by the evaporation of the cloud, ceases in a short time to be experienced, and a period then occurs, comparatively warm, when evaporation from the surface may be renewed. But, in the absence of cloud, radiation cools the surface, and acts with increasing effect upon it until some time before sunrise, when the surface is cooled to the greatest extent by radiation, and much dew is deposited. But *morning*, thus separated from *evening dew*, by the time of its formation, is produced by the cooling influence of radiation from the surface of the earth; whilst the *falling evening* dew is descended cloud brought down by the cold of cloud evaporation.

The circumstances favourable to the formation of dew, are, an abundance of aqueous vapour in the air, and a clear sky. The dew-point over the Mediterranean is sometimes as high as 75°, and the transparent vapour of the part so circumstanced will, by diffusion, and the action of gentle breezes, be conveyed to the adjoining countries, where clear skies and great radiation condense large portions of the

vapour as dew, which supplies to a considerable extent the place of rain in supporting vegetation.

It is well known that heat radiates from the surfaces of all substances, but more freely from some than from others. That which radiates from the surface of the earth, when the atmosphere is clear, seems to pass into space, and is lost to our planet; such radiation, therefore, cools the surface of the earth on a clear night. But on a cloudy night, the clouds may return nearly as much heat to the earth as the earth radiates, leaving the temperature almost undisturbed from that cause. Indeed, it is conceivable that at times the radiation from the cloud may be *even greater* than that from the earth. A newly formed cloud is warmer than dry air at the same height, and the radiation from the cloud is proportioned to the temperature; it is therefore conceivable that the earth may be cooled down to a low degree, and then a warm cloud be formed over it, when an excess of radiation from the cloud would warm the earth. But, whatever may be the temperature of the cloud, it will radiate heat downward in proportion to that temperature, and counteract to a greater or less extent the cooling effect of radiation from the surface of the earth: whilst in a clear sky there is no such return of heat, and therefore in such a sky radiation cools the earth's surface.

On the earth being thus cooled to a temperature below the dew-point of the atmosphere, a part of the atmospheric vapour that rests on the earth will be condensed and form dew, as already described. But it is found that this dew does not form equally on all substances, because different substances radiate heat with various degrees of force, and a scale may be formed of these substances, showing their radiating powers—as, say, Wool, Cotton, Down, Grass, Leaves, Sand, Glass, Porcelain, Varnish, Wood, and Metals.

From this scale it appears that the best radiating bodies are organic and silicious, and this is more particularly the

case when they present large surfaces, from which the heat can pass freely. Any thing that tends to compress or condense the substance into a more compact body, injures its radiating power. Thus a loose fleece of wool, if compressed into a comparatively solid mass, will not radiate equally well; whilst polishing a piece of metal will deprive it of a portion of its radiating power.

Why there should be this difference in the radiating powers of substances we do not know. There seems to be some relation between the conducting and radiating properties of various bodies, the best conductors being the worst radiators. The extent of surface also appears to have considerable effect, as the greater the surface the greater the radiation; hence the large amount of radiation from leaves of trees and grass.

Counter radiation has, however, wherever it takes place, its full degree of effect in producing the general result. The under sides of the upper fibres of loose wool and leaves of trees, like a cloud above the earth, will radiate heat downwards, and to a proportionate extent counteract upward radiation from lower objects. And any contiguous lateral substance will have a similar effect, as it will reciprocate the radiation. A covering of the slightest kind may thus counterbalance the upward radiation.

Radiation of heat is, then, the cause of that cooling of the surface of the earth during the night which takes place under a clear sky, and that produces liquid dew on the earth: but the cooling thus produced is not equally great in all parts under apparently similar circumstances. It is the greatest in the interior of large continents, and more particularly where there is a very dry atmosphere, as in parts of Russia, the Desert of Bokhara, the great Desert of Northern Africa, and other similar parts. Accounts of travellers in such places represent the cold produced in them by radiation during clear nights, as being more intense than in other countries

having a damper atmosphere. The cause of this difference is, however, not to be sought for in any greater clearness of the atmosphere in dry than in damp climates, as the fact is rather the reverse of this, the air being somewhat clearer in the damp countries; but in a process which, when dew is formed, always counteracts to a certain extent the cooling effect of radiation.

When radiation cools the surface of the earth so much as to condense and liquefy some of the aqueous vapour that is in the air, that liquefaction liberates much heat: and this heat tends to warm the part that is in course of being cooled by radiation. There is then a double process going on at the same time and in the same place! Radiation is cooling the part, whilst liquefaction of vapour is warming it; and, under these circumstances, it is only to the extent that the influence of the former exceeds the latter, that cooling is accomplished. When there is much vapour in the atmosphere, much of it is soon liquefied, and the cooling effect of radiation is thereby counteracted to a great extent; when there is little vapour, there is less of liquefaction of that vapour, and cooling is consequently less counteracted. And where the atmosphere is so dry as not to admit the liquefaction of any vapour from the degree of cold that exists, radiation produces its effect without being in any degree counteracted by recently liberated heat.

In such dry deserts as those referred to, the cold in the early part of the night, when it produces dew, produces it only on the best radiators, which are generally the few vegetables that are found in the deserts; and, as the cold increases, worse radiators have dew deposited on them successively in the order of their radiating powers.

In our own country, from the operation of the cause here pointed out, radiation does not produce that intense cold in the early part of the winter, when the dew-point is comparatively high, that it does at a later period of the

season, when the dew-point is very low. In the latter part of the winter, as there is not sufficient vapour to permit much of it to be liquefied by the cold of radiation, that cold may, and frequently does, go on increasing without counteraction during the absence of the sun.

Thus we find that vapour, when condensed into liquid by cold, always gives out heat; whether it is in the formation of the cumulus cloud in the higher regions of the atmosphere, in producing mist near the surface of the earth, or in the production of dewdrops on the surface, the same effect is experienced; and, wherever heat is liberated, it must have its degree of influence in counteracting the cold of radiation.

It has been stated, that " metals give to glass, near which they are placed, the property of more speedily attracting caloric from hot air; and, on the contrary, that of yielding it more speedily to cold air," because a mercurial thermometer accommodates itself to a higher temperature sooner than an air thermometer. But this may be because the heat which passes into the glass tube of the thermometer is rapidly absorbed by the mercury. In like manner, when placed in a colder medium, the heat of the mercury is conducted to the inner surface of the glass tube more rapidly than is the heat of the inclosed air; the mercury therefore cools quicker than the air. But these results are consequences of mercury being a better conductor of heat than air is. And when a piece of foil is placed on the inside of a pane of glass, the outside of the glass opposite the foil is not so soon cooled by radiation, because the metal furnishes heat to supply the place of that lost by radiation from the glass.

In conclusion, we may then say that "*falling dew*" is produced by the descent of the cumulus or day-cloud, which, cooled by evaporation in a higher part of the atmosphere, sinks in the evening to the surface of the earth. *Floating dew* is found in parts which have much vapour in the air in proportion to the temperature, along with a clear atmo-

sphere, and when, consequently, radiation from the surface cools the atmosphere contiguous to it, and condenses a portion of the vapour which the air contains into minute globules of liquid, which are sustained by the elastic force of the air. Whilst *dew*, properly so called—that which is found attached to various substances in the form of drops—is a result of the cooling of certain bodies below the dew-point of the atmosphere by radiation of heat from those bodies, and a consequent condensation and abstraction of some of the vapour which the air resting on them contained. And the more any body is thus cooled, the greater will be the quantity of dew deposited on it. In the two last-mentioned modes, dew supplies to a certain extent the place of rain. Where clouds are freely formed, rain falls on the earth to some extent; but when rain is absent, and the sky is cloudless, radiation of heat, by cooling and condensing vapour, gives some moisture to the earth. Thus the sands of Africa and Asia, which are never visited by rain, have their scanty vegetation supplied with a certain amount of that moisture which is so essential to the life of organized beings.

## IV.—*On Lightning and Lightning Conductors.*
*By* Mr. William Sturgeon.

Read March 21, 1848.*

1. The subjects of lightning and lightning-conductors have long ceased to be novelties in the history of science. Nearly a hundred years have passed away since these interesting topics were first broached in connexion with each other, and they have been objects of great attention amongst the most profound electricians that have appeared within that period.

2. With respect to the element of lightning, philosophers of all countries have agreed that it is of a purely electric origin; but they have differed greatly in their opinions respecting its operations on terrestrial objects, especially on lightning conductors. Nor has this difference of opinion been limited to philosophers of any particular period: it has existed amongst electricians throughout the whole history of lightning conductors; and, although there is one prevailing fashion of conductors in almost every place where they are erected for the protection of buildings—a fashion first recommended by their illustrious author—they have been objected to by electricians of equal standing with Franklin, or perhaps with any other that has appeared interested in their favour; and, as there does not appear to have been any discussion amongst electricians sufficiently profound to set this important question at rest, it would

* The period which has elapsed since this paper was read, has enabled the author to insert the additional instances of lightning discharges described under Cases A, B, and C.

seem as undetermined now as it was the first day it was broached. This is the more to be lamented, because the confidence which ought to be reposed in lightning conductors, notwithstanding the numbers that have been erected, remains imperfectly established: nor is it likely that much reliance will be placed on those in common use, so long as lightning is known to commit its ravages much within the range of their *supposed* protective influence, as prescribed in the theories of philosophers whose names most justly stand in high repute.

3. Mr. Benjamin Wilson, than whom no contemporary electrician was more capable of investigating the subject, nor any one more zealous and indefatigable in arriving at experimental truths connected with it, most strenuously objected to the pointed conductors of Franklin; and, although his views were disregarded by a committee of the Royal Society when lightning-rods were proposed for the Purfleet magazines, it is a remarkable fact that the most valuable experiments brought forward on that memorable occasion were those made by Mr. Wilson himself; and which tended as decidedly to confirm the justness of his own views respecting pointed conductors, as any that were produced in favour of them.

4. At the time this controversy was going on, the subject was comparatively new, its introduction to the scientific world being recent, and no discussion of importance having previously taken place. Moreover, at that period philosophers had no means of ascertaining, from actual observation on an extensive scale, the influence of vertical conductors in giving direction to strokes of lightning, or of preventing them altogether; their only guide being a few experimental facts, which were too limited in the range of their action to establish a theory sufficiently comprehensive to embrace every topic concerned in the investigation. Since that

period, however, the effects of lightning have been more closely attended to, and minutely observed, and an abundance of data respecting the influence of pointed conductors, unknown to Franklin and his contemporaries, have been collected, which furnish new arguments in the discussion, and open a more expanded view and a clearer aspect of the whole subject, than any that could possibly be laid open to the famous electricians of the last century.

5. Under these circumstances, there is much reason for supposing, that such new arguments as are derivable from facts, and susceptible of legitimate introduction to the theory of lightning conductors, would not only effect an important step in the advancement of electricity, but might lead to advantages of great consequence in the practical application of its principles in the protection of persons and property from the effects of lightning. Moreover, as her Majesty's palace, Osborne House, has suffered from lightning within the last year, although eight tall pointed conductors were attached to different parts of the building at the time, the subject has derived a new interest, and of sufficient import to recall the attention of philosophers to its investigation; and it is with a view of conducing to the usefulness of this branch of physics, that this *Review* is respectfully submitted to the consideration of this Society.

6. The principal topics unknown to Franklin and his contemporaries arise from events of comparatively recent date, and supply arguments of high importance to electricians of the present day. The number of instances in which lightning has struck objects close to tall pointed conductors, whilst others, situated at a greater distance, and equally exposed, have escaped injury—is a fact that requires the deepest consideration; and a rigid investigation for ascertaining the cause is imperatively demanded at this moment.

7. The well-known cases at Purfleet, Tenterden, and

Heckingham, in which lightning struck buildings to which pointed conductors were attached, made very little impression on the minds of those philosophers whose doctrines might have been thrown into jeopardy by a careful investigation of all the circumstances connected with these important events. They were considered as cases of easy explanation, by admitting imperfections of conduction from metal to metal, or from the conductors to the ground. But the abundance of similar cases of more recent date, in none of which is such a subterfuge to be found, will neither allow of a summary disposal, nor the evasion of a rigid enquiry, for the mere purpose of giving countenance to any favourite doctrine whatever. They are events of vital importance to society, both at sea and on shore; and form a topic of the highest consequence to which philosophers can at this time devote their attention.

The following cases will be sufficient of themselves to call immediate attention to this class of events. The first two described have come under my own observation; and the others are from authentic records.

8. CASE A.—About half-past eleven in the forenoon of Saturday, June 3, 1848, a flash of lightning struck a stack of chimneys on the *Northumberland Arms* public-house, Chester-road, Manchester. I visited the house a few days after the occurrence in company with two scientific friends, Mr. E. W. Binney and Mr. John Leigh; and obtained a full account of the circumstances attending the event from the landlord's brother, who was on the premises at the time it happened. The stack of chimneys struck by the lightning is precisely five yards from a lightning conductor, which is attached to another similar stack of chimneys, and above the summit of which it rises about two yards, terminating upwards in several diverging metallic points.

The particular chimney that was struck belongs to the kitchen, the only room in which there was a fire at the

time. The chimney-pot was broken, a quantity of bricks thrown down, and a large coping-stone displaced. The lightning proceeded down the chimney, and was seen to pass through the fire-grate. The soot filled the kitchen in a moment, and nearly suffocated all who were within, consisting of the landlady, and several persons who had taken shelter from the heavy rain then falling; and a bundle of clothes belonging to one of them was blown from the kitchen floor to the passage leading to the street door. When the lightning left the fire-grate, it fell upon a gas-tube which supplied a burner close to the mantelpiece; thence it was led to the *main* (an iron tube), in the cellar beneath. From this tube it burst and blew off one of the brass coupling-joints, and set the gas on fire, which would probably have set the whole premises on fire, had my informant not seen it in time, and fortunately had presence of mind to turn the tap, which cut off the supply. The lightning made its exit by means of the gas retort and other metallic apparatus in connexion.

9. Case B.—On Thursday, August 16, 1849, a discharge of lightning fell upon the top of a tall chimney belonging to the works of William Collier & Co., machine-makers, Salford. The chimney stands close to the river Irwell, directly opposite to the Manchester Cathedral; and was armed with a conductor at the time. The conductor consists of a series of stout iron gas-tubes, screwed together in the usual way; and its upper end was *then* furnished with several diverging metallic points—a prevailing fashion in Manchester. The brick-work of the chimney was surmounted by heavy coping-stones, held together by a hoop of cylindrical rod-iron, which lay in a circular groove, or bed, cut in the upper surface of the stones for its reception. The conductor passed through the projecting moulding of one of these coping-stones, and rose to about five feet above the summit of the chimney.

The lightning struck the coping of the chimney at the distance of two or three feet from the conductor, and shattered into fragments two of the large stones; many of the fragments were thrown down on the roof of the building, and some of the large ones made their way through the water cistern, and fell into the engine-room. Being acquainted with Mr. Collier, I had every facility afforded for obtaining the full particulars of the accident whilst visiting the building, and examining the chimney and conductor.

NOTE.—Whilst the scaffolding was up for repairing the chimney, the upper part of the conductor was removed, and replaced by a cylindrical copper-rod, terminating upwards in the shape of a sharp spear-head. Not the slightest trace of the effects of lightning could be discovered on any of the old metallic points.

10. CASE C.—Her Majesty's palace, Osborne House, was struck by lightning, June 8, 1849, having eight tall sharp-pointed conductors attached to different parts of the building at the time of the occurrence. The accompanying plan* of the palace shows the positions of those conductors, and also the positions of the cast-iron water-pipes, reaching from the roof to the drains in the ground beneath. Some of these water-pipes have been made available as portions of the conductors, the upper parts of which consist of stout copper-rods, reaching several feet above the roof.

The conductor on the top of the flag-staff tower rises 118 feet above the terrace level, and is 20 feet taller than the tower itself. It is also upwards of 30 feet taller than the clock-tower that was struck by the lightning; but from the latter its distance is about 220 feet.

The clock-tower, as will be seen in the *elevation* accompanying the ground plan, was struck at the highest point of

* The plan referred to has not been found amongst the papers of the deceased author; it has, however, been considered advisable to follow the original memoir.

one of the angles. The lightning descended along the tower about 49 feet, and afterwards made its way across the roof of an attached building, in order to arrive at a cast-iron water-pipe, by means of which it found its way to the ground. The distance between this tube and the angle of the tower first struck, is about 56 feet. The distance between the nearest lightning-rod and the angle struck, is about 152 feet; but as the roof of the building to which that conductor is attached, is 34 feet lower than the top of the tower, the latter is considerably more lofty than the point of the conductor.

11. CASE D.—The powder magazine at Bayonne, which was guarded by a conductor of the most perfect kind that the philosophy of France could suggest, was injured by lightning, February 23, 1829. The building is about $17\frac{1}{2}$ yards long, and about 12 yards broad. Its gables are covered with large plates of lead securely joined together, and to others which cover the ridge of the roof. The gutters around the roof are also of the same metal. The conductor, which was an iron rod, pointed at its upper end,* rose to the height of about 19 feet above the roof. It passed through a leaden socket, attached by solder to one of the leaden plates that covered the ridge of the roof, by which means the whole of the metallic parts of the roof were in communication with the conductor.

The lower part of the conductor, instead of proceeding into the ground at the foot of the wall, in the usual way, was bent at right angles about $2\frac{1}{2}$ feet above the ground, and continued in a horizontal direction, supported on wooden posts, to the distance of about 10 or 11 yards, where it was again bent at right angles downwards, and entered a pit two yards square, partially filled with bruised charcoal. The sides of the pit were sustained by masonry, having

* As the Royal Academy of Science had recommended the use of platinum wire for the superior termination of conductors, it is probable that this conductor was surmounted in that manner.

plenty of arched openings near the bottom for allowing of free conduction from the charcoal to the moist earth. In order to expand the metallic conduction amongst the charcoal, the lower end of the conducting bar was furnished with a number of pointed metallic rods, which ramified into every part of the carbonaceous mass. The pit above the charcoal was filled up with loose earth, which was covered with stone flags.

12. The lightning appears to have struck at two points, one of which was the point of the conductor, which it fused, and the other was one of the leaden plates that covered the opposite gable. This plate was severely rent in different directions, and the leaden caps of the posts which supported the conductor in its horizontal direction were also torn, and some of them doubled up, and the nails which had held them to the wooden posts were drawn.*

13. Were there no other cases known than those above described, they alone would be sufficient to testify the incapability of pointed conductors, of the usual form, of affording protection to buildings to which they are attached; and to disqualify them as guards against the attacks of lightning even close to their own posts. The cases A and B are probably the most remarkable on record, as regards the vicinity of conductors to the points struck by the lightning. They afford indubitable evidence that lightning will approach those conductors, and commit its ravages within a few feet of them, although armed with many finely-pointed branches diverging from one another, and piercing the contiguous air in almost every direction.

14. It cannot be supposed, however, that the lightning in these cases passed through the air directly over the points of the conductors, and shunned them in order to fall on some other point of its circuit. Such an idea would have

* *Annuaire* for 1838.

to be formed in direct opposition to the well-established laws of electricity; but it might easily be made to appear probable that those pointed conductors were conducive to the lightning's approach from the opposite side, and yet incapable of affording sufficient facility for securing its arrival at their elevated extremities.

15. In the case of the tall chimney (9), there is every reason to suppose that, after striking the coping-stones, or the iron hoop which lay upon them, the lightning found its way by explosion to the conductor, and was thence quietly conveyed to the ground. But there is no evidence whatever, that any portion of the lightning entered the points with which the conductor was armed.

16. At the *Northumberland Arms* (8), the case was somewhat different, there being no indication whatever of any portion of the lightning having entered the conductor at all. The whole force of the discharge seems to have been directed through the chimney flue to the fire grate, and thence by means of the gas apparatus to the ground. In both cases, the lightning seems to have made its approach on the opposite sides of the objects damaged to those on which the conductors were situated.

17. The leaden plate that was struck on the magazine at Bayonne (11), was obviously a portion of the conducting metallic mass belonging to the lightning-rod; and, although the damage at that part of the building might have been effected by a discharge perfectly distinct from that which fused the point of the conductor, the transmission of the electric fluid between the roof and the ground would be through the same conducting channel, excepting such portions as would flow over the surface of the wet walls. But in this case, as well as in the cases A and B, there is much reason to suppose that the tall pointed conductor was conducive to the lightning's approach, and consequently to the event. Nor am I less disposed to attribute the effects of

lightning on the clock tower of Osborne Palace, to the forest of pointed conductors in its vicinity. Those conductors, as well as the fine tall trees that are about the building, will always be the means of giving a tendency to their locality for the reception of discharges of lightning; and that tendency will necessarily be increased in some proportion with the number and altitude of pointed conductors on or about the building. Such at least appears to be the natural inferences derivable from an association of the well-established influence of pointed conducting bodies, with many observed circumstances attending discharges of lightning.

18. Damage by lightning in the vicinity of pointed conductors at sea, is no less remarkable nor less frequent than on shore. There are many striking instances of this kind on record, some of which afford lessons of no ordinary import.

19. CASE E.—In January 1824, H. M. ship *Milford*, 74 guns, was struck by lightning within the distance of 80 fathoms from the *Caledonia*, of 120 guns, and several other ships close at hand, all of which had pointed conductors up at the time. A powder magazine on shore, at no great distance, had also a conductor attached to it; and the report says, that this conductor was in the direction from which the thunder-gust proceeded.

In this case the damaged ship was *lying in ordinary*, without conductors, in Plymouth harbour. The Caledonia had a conductor at each mast; but neither that ship, nor any others which had their conductors in place, received any portion of the lightning.*

20. CASE F.—H. M. ship *Phaeton*, 46 guns, whilst in Gibraltar Bay, in the year 1824, was much damaged by lightning, at a cable's length from the *Warrior*, the latter having pointed conductors up at the time.†

21. CASE G.—H. M. ship *Pelican*, 18 guns, was struck and

* *Nautical Magazine*—Harris on Thunder Storms. † Ibid.

much damaged by lightning, whilst on the coast of Africa, and at a short distance from the *Waterwitch*, the latter vessel having her pointed conductors in place at the time.*

22. Case H.—H. M. ship *Ceylon* was struck by lightning in the year 1838, whilst lying in Malta harbour, and at a short distance from the *Talavera*, *Bellerophon*, and *Hastings*, three line-of-battle ships, and fully rigged and equipped with conductors. The *Ceylon*, as a receiving-ship, had only a short pole above her fore-mast, whereas the other ships being fully rigged, their masts and conductors were above 150 feet up into the air.†

23. Case I.—In 1815, H. M. ship *Norgé* was severely damaged by lightning; whilst the *Warrior*, 74 guns, with a pointed conductor, lying close to the *Norgé*, received no injury. Many other ships with *conductors* were in the same harbour at the time; they all escaped but the *Norgé*, which had no conductor.‡

24. Case K.—On the 25th of March, 1840, H. M. ship *Powerful*, of 84 guns, was struck by lightning whilst at anchor at a short distance from the *Asia*, also an 84 gun ship, and furnished with fixed conductors in her masts. The *Powerful* had no conductors. ‖

25. The four following cases show that lightning occasionally falls into the sea close to tall-masted vessels, notwithstanding their being armed with pointed conductors; which, according to the views of Franklin and his followers, ought to prevent such vicinal explosions.

26. Case L.—On the 21st of January, 1840, a discharge of lightning fell into the sea so near to the *Neptune*, a small revenue cutter at anchor in Ely Bay, as to cause the vessel fairly to reel by the concussion.§

* *Nautical Magazine*—Harris on Thunder Storms.

† Ibid. ‡ Ibid. ‖ Ibid. Also, Parliamentary Return of Ships struck by lightning.

§ Harris on Thunder Storms.

27. CASE M.—In the month of June, 1840, a discharge of lightning fell so near to H. M. ship *Southampton*, of 50 guns, that it appeared to strike the main-chains.*

This vessel had fixed pointed conductors in all her masts at the time of the occurrence, they having been applied two years previously.†

28. CASE N.—In the year 1840, a dense explosion of lightning fell close upon the quarter of H. M. ship *Vanguard*, of 80 guns, whilst proceeding from Portsmouth to the Mediterranean.‡

29. CASE O.—A discharge of lightning fell close to the *Dart*, a steam-packet, whilst on her passage from London to Margate.||

30. The most interesting of the last four cases is that of the *Southampton* (Case M.), because of the certainty we have of the presence of three of those conductors upon which so much confidence is now placed for protection; and of the fact that the lightning, notwithstanding its near approach, disregarded their conduction, their points, and their prominence, and found an easier transit to its destination close to the side of the ship. It is one of those events that would lead to the inference, that tall pointed conductors may facilitate discharges of lightning in their own direction, though incapable of preventing their taking another route when within a certain distance of them. Moreover, analogy would lead to the inference, that although the *Southampton* fortunately escaped, it is possible that such conductors might be the means of lightning falling on the deck of the vessel to which they were attached. Nor does such an inference rest on mere probability, it being already verified by the following well-authenticated facts:—

31. CASE P.—In March 1848, at Calcutta, H. M. ship

* Harris on Thunder Storms.

† *Parliamentary Return* for 1849.

‡ Harris on Thunder Storms.

|| Ibid.

*Endymion* was struck by lightning on the fore-topgallant mast, at the distance of about 50 feet from a pointed conductor attached to the main-mast, reaching from the topgallant-mast's head to the water.*

32. CASE Q.—H. M. ship *Ætna* was struck by lightning near the bow, which exploded about 12 feet above the forecastle, close to the fore-mast, whilst a chain conductor was attached to her main-mast in the usual way.†

33. CASE R.—In May, 1835, at the Cape of Good Hope, H. M. brig *Racer* was struck by lightning on the fore-topgallant-mast, a chain conductor being in its place on the main-mast at the time.‡

34. Although more instances of this kind might be adduced, those already cited are sufficient of themselves to show the fallacy of that doctrine, which embraces the idea that tall pointed conductors will prevent violent explosions of lightning from falling on vicinal objects. They, moreover, prove that lightning does not invariably select the tallest objects for its transit to the earth; which is another fact at variance with the views of Franklin, and the prevailing opinion at the present day.

35. I am not aware that *oblique* discharges of lightning had ever been noticed by writers on electricity previously to the appearance of my Memoir on *Marine Lightning Conductors*, dated September, 1839,‖ although the character of the damage by lightning, in many instances, was too obvious to lead to any other conclusion. It is even doubtful that any lightning discharge takes place in a vertical direction. If lightning invariably took a perpendicular direction, it would as constantly strike the highest points of such objects as ship's masts, &c., which is contrary to observation. Nor

* Harris on Thunder Storms. † Ibid.

‡ *Nautical Magazine*—Harris on Thunder Storms.

‖ *Annals of Electricity*, vol. iv.

does it invariably strike the nearest objects, as was supposed by Franklin; for in many cases it strikes trees obliquely, which are surrounded by objects much taller than themselves, as well as the rigging and masts of ships at a short distance above deck.

36. In illustration of damage to ships by oblique discharges, we may refer to Harris's "*State of the Question relating to the Protection of the British Navy from Lightning*," in which work I find, that out of one hundred and seventy-four cases of ships being struck by lightning, "the particulars of which have been ascertained," there are not more than forty-four in which the topgallant-masts have suffered; consequently, in the remaining one hundred and thirty cases the lightning struck no higher than the topmasts; and as it is highly probable that in most of these cases the topgallant-masts were standing, the lightning must have approached the ships in oblique directions.

37. The best and most obvious illustration of the effects of an oblique discharge of lightning that has come under my own observation, was in the case of St. Michael's Church, Liverpool, which was struck by lightning about two o'clock on Tuesday morning, August 23, 1841. The damage to the beautiful spire of this church was so great on this occasion, as to require it to be taken down entirely; and when I heard that the scaffolding for that purpose was completed, I proceeded from Manchester to Liverpool to examine the effects of the lightning. On my arrival at the top of the spire I first examined the metallic cross, and the ball in which the lower end of the shank of it was fixed, and soon discovered, by the discoloration of the metal, which was of bronze, richly gilded, that the electric fluid had first struck the lower end of the shank of the cross, and that it had not touched the upper part of it at all. The ball or cap, and cross together, were 9 feet 6 inches high, and I estimated the cap, which was a hollow globe of

bronze, to be about 2 feet and a half diameter, which would leave 7 feet for the height of the cross above the surface of the cap.

A more decisive indication of the precise spot where the electric fluid first struck the cross, could not possibly have been marked than by the well-defined boundary of discoloration of the bronze. And, what was still more satisfactory, the *obliquity* of the upper boundary line of discoloration showed that the cloud from which the lightning proceeded was, at the time of the discharge, on the north-west side of the church. For on that side of the shank of the cross was the *highest point* of the margin of discoloration, and on the opposite side was the lowest point of that margin; and the figure of this upper margin or boundary line was that of an ellipse, embracing the shank of the cross, and sloping downwards from the north-west to the south-east side.

The highest point of this margin was not quite a foot and a half above the surface of the globular cap, leaving more than 5 feet of the best conducting metal known, above that point, untouched by the lightning. The gilt bronze, from that boundary line down to the horizontal equator of the globular cap, was converted into a leaden colour. At the equator the discoloration became scattered and lost in a multitude of ramifications on the lower hemisphere; these ramifications were, with the exception of colour, very similar to those made on the surface of a glass jar by spontaneous discharges. Such were the external effects of the lightning on this mass of hollow bronze metal, the conduction of which was assisted by an interior bar of copper, which supported it on the apex of the spire of masonry. The same copper bar reached downwards in the axis of the spire nearly 40 feet, and rested on the intersection of two horizontal bars of iron, placed at right angles to each other, having their extremities fixed in the masonry.

The stones of this spire were held together by copper cramps secured by lead, the best plan possible for insuring its destruction by lightning: and the facilities for such an event were still further increased by the lodgement of long strips of lead amongst the stones, which had been poured into the crevices whilst fastening the cramps. The upper part of the spire was so unstable at the time I was examining it, that it vibrated with every movement of the foot on the uppermost stage of the scaffold, and a blow by the hand on the shank of the cross would make the whole spire tremble. The principal rents which the lightning had made in the spire, were diametrically opposite to each other; some of them were about 24 feet long, but not very wide. I took several of the strips of lead out of the damaged masonry, some of which weighed a pound and a half.

38. Oblique discharges of lightning being phenomena of comparatively recent notice, the electricians of the last century had no means of contemplating their effects. They form a novel feature in Atmospheric Electricity, and, in a practical point of view, are of vital consideration. In the Memoir already alluded to (35), I pointed out the probable consequences of oblique discharges of lightning when falling on the rigging of vessels in the masts of which are fixed conductors, in the manner adopted in the Royal Navy. The following are the remarks I then made:—" It will be obvious that an *oblique* flash of lightning striking into the rigging could not arrive at a conductor let into the *after-side* of the mast, without damaging the mast itself, unless it proceeded from a cloud astern* the vessel. Were lightning to strike any of the yard-arms in order to arrive at a conductor, that yard-arm would receive as much damage as if no conductor had been in the mast; and it is

* This expression was intended to comprehend the whole of the arch abaft the beam.

even possible that the conductor would be the means of increasing the damage, by causing the lightning to run along the whole length of the yard-arm to the mast, and the mast itself might then be traversed by the lightning, and shattered between the yard and the conductor. The sails, ropes, &c., and every article which the lightning met with on its way to the mast, would suffer damage to precisely the same extent as if no conductor were attached to it; and men placed in, or near, the track of the lightning, would be as sure to meet a death-blow as under any other circumstances in which lightning entered the rigging. Moreover, as these *central* conductors would offer increased facilities for lightning to strike the masts, all the evils usually attending oblique discharges through the rigging to them, would necessarily be increased also."

39. In confirmation of the justness of the views thus recorded (35, 38), there has subsequently occurred a remarkably prominent case, in which the rigging of H.M. ship *Dido* was struck by lightning, in May, 1847, whilst on her passage to New Zealand. "It appears by the accounts from the ship, that soon after daylight, there being at the time heavy rain, with little wind, thunder and lightning, a vivid and fierce discharge fell aloft, in a double or forked current, upon the main-royal-mast; *one of the branches struck the extreme point of the royal yard-arm, and in its course to the conductor on the mast, demolished the yard, and tore in small pieces, or scorched up, the greater part of the sail.* The other part fell on the vane, spindle, and truck, which last was split open at the instant of the discharge seizing on the conductor." *

By admitting, in this case, that the discharge bifurcated, as has been supposed by the author of the work referred to,

* Harris's "Remarkable Examples of the operation of capacious metallic conductors, permanently fixed throughout the masts and hull, in *defending* H. M. ships from the destructive agency of lightning."

and consequently that the yard-arm was struck by a *portion* only of the whole force, there is much reason to dread the effects of a full and complete oblique discharge of lightning amongst the rigging of a vessel on which it falls.

40. Another case of oblique discharge occurred to H. M. ship *Fisguard,* 42 guns, September 29, 1846, whilst at anchor at the mouth of the Nisqually river, in the Oregon territory. In this case the lightning struck the main-mast only a few feet above deck. This ship was furnished with a conductor in each mast at the time. The indications of this oblique discharge were *ruptures* in the conductor, and its being *started* from the mast; all of which occurred within 13 feet above deck. The places at which the conductor was started from the mast, were respectively at "twelve and a half, seven and a half, and two and a half feet above the upper deck. The plates of copper forming the conductor were *separated* at the lowest point, and thrust, as it were, asunder: the edge of the groove in which the plates were laid, was slightly rent by the starting of the plates, thereby causing two or three splinters to fall on the deck at the time of the discharge."* It is also stated that the mast was "*slightly singed*" at the lower mast point, or where the copper plates were separated; hence there is reason to infer that this was the principal point on which the lightning struck the mast; and the probability of this being in reality that which actually took place, is strongly supported by the fact, "that several boarding-pikes ranged round the mainmast were displaced, and their wooden stand *slightly charred*."†

41. The track of the lightning in this case (40), would be within the height of an ordinary sized man before it arrived at the mast; hence the probability of a great sacrifice of life had men been standing on the same side of the mast at

* Harris's "Remarkable Examples," &c.

† Ibid. (Captain Duntze's Official Report.)

the time it was struck. Had the lightning not entered the boundaries of the rigging *abaft* the main-shrouds, they, and other parts of the cordage, would probably have suffered to a very great extent; and, had the ship's sails been spread, the damage from such a flash of lightning might have extended to a great part of the rigging.

42. With respect to the ruptures in the conductor, and its being *started* from the mast, they were results that had been foreseen, and were clearly pointed out as probable occurrences, in the *Memoir on Marine Lightning Conductors*, previously noticed (36); in which it is stated, that "a conducting strip of copper, close jammed to the wood, within a groove in the mast, might probably not only be burst asunder, but peeled from the wood for many feet upwards and downwards, from the point where the lightning struck the mast."* The correctness of this view of the effects of lightning on conductors fitted into grooves in the masts, has been further verified on H. M. ship *Scylla*, 18 guns, which was struck by lightning on the main-royal-mast, August 16, 1843, whilst on the West India station. It appears that, in this case, the mast was struck below the truck by an oblique flash; and that "some of the butts of the copper plates (forming the conductor) in the topgallant-mast were *started*, and in one place *buckled up* at the edges; some of the fixings also were shook and loosened."†

43. In addition to the remarks already made respecting the inefficiency of pointed conductors (13), the three cases last described (39—42), require particular attention; because each ship had its complement of conductors, one in each mast, in place, at the time they were respectively struck by lightning; and, as the whole of the royal-masts were standing, there was every chance afforded for the pointed extremities of the conductors to ward off the explosions. The

* See my "Scientific Researches," quarto, p. 363.

† Harris's "Remarkable Examples," &c.

actual effects, however, have proved to demonstration the fallacy of that doctrine, which rests solely, or principally, on the influence of points in conferring efficiency on lightning-rods, and rendering them protective, either by dispersing a thunder-storm or mitigating its effects; and have shown that no protection whatever is to be expected by an exposure of points at the superior extremities of conductors, beyond that which would be afforded by any other form of termination.

In the case of the *Dido*, the lightning struck a yard-arm in preference to any of the three pointed conductors, which pierced the air at a much higher altitude: and in the *Fisguard*, the fore and mizen conductors were disregarded by the lightning, which found an easier route to the sea by striking the main-mast at only a few feet above the deck. And, what is very remarkable in this case, the lightning obviously entered the boundaries of the rigging *abaft the beam*, and sloping downwards from the cloud, it would necessarily have to pass, at no great distance, the point of the mizen conductor, unless the obliquity of its path was very great indeed.

44. Nor have these cases shown that pointed conductors have any power in abating the force of lightning discharges which assail the objects to which they are attached; for the damage was as extensive as could possibly be supposed to occur, whatever might have been the form in which the conductors terminated; and the explosion in every case was terrific, especially that which occurred to the *Fisguard*, which, in Lieutenant Rodd's letter, is said to have been "beyond all description;" and Lieutenant Dyke says, "the crash was most awful, just as if five hundred broadsides had all gone off together."* It is also stated, that "the officers who saw the lightning strike, all agree in the fact of the mast being apparently wrapped in a blaze of electric fire."†

* Harris's "Remarkable Examples," &c.

† London Illustrated News. Private Letter from the Fisguard.

The boatswain's mate was "blinded at the moment by the intense light, and knocked down on the deck."* "Below deck, whilst the lightning was traversing the branch conductors, every one in the ship was stunned and amazed by the noise and concussion of the explosion."† "Some of the branch conductors at the points of contact with the iron knees were blackened, and the copper bands which covered the exterior ends of the through bolts were started, and the copper sheathing laid over them was bulged outwards."‡

45. The doctrine which sets forth that "these conductors, by rendering the whole mass, together with the masts, so uniformly conducting in every part, that a discharge of electric matter falling on the mast would thereby lose its *explosive form of action constituting lightning;* and being converted into a comparatively *quiescent current*, traversing capacious metallic conductors, would become dispersed upon the sea without intermediate explosion or damage,"—seems not to be sustainable by experience; since in no well authenticated occurrence of lightning striking ships furnished with this plan of conductors, have the points either warded off the blow, or prevented explosion and damage.

46. Pointed conductors, however, being those in general esteem, and now employed to an unprecedented extent, both at sea and on shore, a just knowledge of their absolute capabilities in influencing electric clouds and the intervening air, has become of more importance at this time than at any former period of their history.

47. To infer that, because a pointed wire, at the distance of a few inches, would neutralize the electric charge of a small metallic body, a similar influence would be exerted on a distant cloud, required a fertility of conception and an

* Harris's "Remarkable Examples," &c.

† Nautical Standard, &c.

‡ Harris's "Remarkable Examples," &c.

amplitude of imagination not usually displayed in profound philosophical contemplations; considering also that the hypothesis implies not only an identity in the material and structure of the bodies operated on, but also an invariability of influence and action, irrespective of distance, it can be viewed in no other light than as one of the boldest steps in the advance of fact hitherto recorded in the history of science. Such, however, is the only basis on which the doctrine of *pointed lightning-rods* was originally founded, and such alone is the source of all the reputation they have acquired, and of all the faith that has been placed in them for protection, from their first introduction to the present day.* Nevertheless, too much praise cannot be awarded to Franklin for this noble attempt to enlist the principles of science into the service of humanity; and although, from the then infantile state of atmospheric electricity, and the consequent want of information necessary for affording correct notions respecting the influence of thunder-clouds on pointed conductors at the earth's surface, and the preliminaries requisite to give direction to strokes of lightning, it must be acknowledged that the idea of protection from lightning by means of vertical metallic rods originated with that eminent philosopher. Had experience in atmospheric electricity been more ample and varied at the time Franklin projected his lightning conductors, he would have known that the atmosphere is replete with the electrical element, independently of the presence of cloud, under every circumstance of serene weather, and in every season of the year; and that those clouds which hang within reach of a kite-string, as well as those which drag upon the ground, are invariably highly charged with electro-positive action;

* It would appear from Franklin's own account of his experiments with pointed bodies, such as sharp pins, that the distance between the electrized body and the point, never exceeded much above 12 inches.—See his *Letters*, or *Philosophical Papers*.

and that ten thousand times ten thousand pointed wires immersed in them, would never perceptibly lessen their electric intensity.

48. Every electrician of eminence knows that fog-clouds are invariably electro-positive with respect to the ground; and those who have explored them extensively can attest that their electric action remains but little, if at all, altered for many successive hours, and, in some cases, for whole days and nights, notwithstanding their exposure to pointed wires, and to the myriads of vegetable points and sharp edges in connexion with the ground, most of which possess a discharging influence equal to that of the finest-pointed needle.

49. Facts like these, had they been collected in due time, would have furnished the eminent American philosopher with views, respecting the capabilities of pointed rods, of a very different aspect to those few on which he reared an hypothesis, perhaps the most dangerous in practical science.

50. The difference in the structure of the prime conductor of an electrical machine, or of any metallic body, and that of a thunder-cloud, is too obvious to need description. The extent also, as well as the continuous formation of the latter, finds no analogy in the former. What myriads of aqueous particles, each of which is a distinct individual conductor, would have to be discharged by a vicinal point of a wire, before those at a greater distance in a cloud could suffer any change whatever; and those still more remote, notwithstanding their tendency to dispose of the electric fluid they contained, in the same direction, would suffer no further alteration than electro-polar arrangement, whilst the still more distant localities of the cloud would remain totally unaffected. Such being the only inference that can be legitimately drawn from the facts, that have been collected from the observations of every electrician who has made the requisite explorations, there remains but little hope of dispelling lightning storms by pointed conductors. And from

what has already been stated, and from other well-attested facts that will appear in the sequel, there is no reason to suppose that such a fashion of conductors has ever been conducive to lessen either the violence of thunder-storms, or of an individual flash of lightning.

51. Franklin's limited experience in atmospheric electricity could not be expected to afford very profound ideas respecting the electric condition of thunder-clouds, on which account he wisely hesitated in giving a decided opinion, although he was led to suppose that, although they are occasionally *positive*, they are more frequently in a *negative* state.* With respect to his pointed conductors, however, it appeared to him to be a matter of no consequence in what direction the lightning traversed them, as they would be equally efficient whether it proceeded from the clouds or from the ground. Had these views been correct, we should never have heard of lightning explosions taking place upon those conductors below their highest points; because, as they are exposed on every side to the non-conducting air, or to bodies of very inferior conduction to themselves, electrical discharges from the ground would traverse their whole length, and invariably quit them at their highest points. Moreover, as the electric element would select the best conductors, and escape most freely from those with sharp terminations, comparatively low objects, situated close to tall-pointed conductors, would not be liable to suffer from *upward* discharges of lightning. But since it is well known that lightning frequently strikes pointed conductors much below their upper extremities, as well as comparatively low objects vicinal to them, it is obvious that in those cases the discharges proceeded *from* the clouds, and that the efficiency of pointed conductors in guiding strokes of lightning, and preventing damage, is much less perfect for *downward* than for *upward* discharges.

* Philosophical Letters and Papers.

## V.—*Researches into the Identity of the Existences or Forces of Light, Heat, Electricity, Magnetism, and Gravitation.* *By* John Goodman, M.D.

Read April 4, 1848.

In a former paper read before this Society, I introduced an arrangement (described in the Report of the British Association for 1847) by which the powerful substance potassium, assists in forming a very quiet and practical voltaic battery. Being a substance possessing the highest chemical affinities, it was also shown to manifest the most exalted electrical energy, and to be capable of decomposing water, and deflecting gold-leaf, by a single pair.

An unanswerable argument was thereby adduced, and a still further corroboration of the *analogy* of all chemical and electrical phenomena, at first indicated by Sir H. Davy.

It becomes now a matter of enquiry, if chemical affinity and electricity are analogical—how far the calorific properties of potassium are in harmony with its other powers?

Whether, if chemical affinity is electrical force—and electricity is only a modified form of caloric (as several experiments have led me to suspect)—the quantity of caloric existing at all times in this extraordinary metal, and the calorific phenomena displayed in its combustion, and in other test experiments, are in any manner proportional to its intense electro-chemical powers.

The first method of procedure which I adopted was, to ascertain the known properties of potassium. This sub-

stance, it will be remembered, was discovered by Sir H. Davy in October, 1807. Dr. Ure gives the following description of its properties:—

*a.* It is lighter than water, being sp. gr. 0·865.

*b.* At common temperatures it is solid, soft, and easily moulded by the fingers.

*c.* At 150 Fahrenheit it fuses; *d.* and in a heat a little below redness it rises into vapour.

*e.* When newly cut its colour is splendent white, like that of silver; *f.* but it rapidly tarnishes in the air; *g.* to preserve it unchanged, we must enclose it in a small phial with pure naphtha.

*h.* It conducts electricity like the common metals.

*i.* When thrown upon water it acts with great violence, and swims upon the surface, burning with a beautiful light of a red colour mixed with violet.

*k.* When heated moderately in common air it inflames, burns with a red light, and throws off alkaline fumes; *l.* placed in chlorine, it spontaneously burns with great brilliancy.

*m.* The attraction of chlorine for potassium is much stronger even than the attraction of oxygen for this metal.

*n.* Lastly, of *all* known substances potassium is that which has the strongest attraction for oxygen, and it produces such a condensation of it, that *the oxides of potassium are denser than the metal itself.*

Dr. Faraday has also shown, that after the addition of oxygen and iodine to this substance, the resulting salt is of less volume, and occupies less space, than the original bulk of the potassium itself.

The vivid and intense light and heat given off by the union of potassium with chlorine, oxygen, sulphur, and phosphorus, seemed to me to exhibit, that this metal possesses a large amount of absolute heat. The intense *affinity* of this substance for electro-*negative* bodies appeared also to be

a corroboration; and my friend Mr. Joule has shown, that considerably more heat is given off during the combustion of a few grains of potassium, in oxygen gas, than by a like quantity of any of the other metals, zinc, iron, &c., and that the quantity given off by each metal, is in harmony and proportion with their electrical powers in voltaic arrangements.

*Experiment* 1.—I found that when potassium is thrown into a red hot crucible, it explodes, and produces a small flash and sound, much resembling the deflagration of the priming of a gun. Held in the flame of common gas, it burns still more vividly, and throws off bright scintillations like iron wire in oxygen gas.

*Exp.* 2.—When placed in a tube of metal or of glass, hermetically sealed and submitted to the operation of the blowpipe, at a given temperature it begins to force its way out in vapour, and burns at the orifice, like charcoal and nitrate of potash, with a hissing noise.

### ABSOLUTE HEAT.

Attempts have frequently been made by philosophers to find out the absolute caloric contained in different substances. It scarce need be added, that the intricacy of such a subject has rendered great discrepancy in their results, and shown their utter inability ever to grapple with it.

But although it does not appear to appertain to man to ascertain the absolute quantities, volume, weight, form, and distance of the forces and nature of first principles or elements, yet *relative* properties and quantities are to a great extent within the grasp of philosophical research.

### COMPRESSION OF POTASSIUM.

*Exp.* 3.—The metal was now subjected to compression, in order to exhibit the heat given off during the period in which the elementary particles are supposed to be forced

nearer together, and whilst the specific gravity is being increased. This experiment was devised after the manner of the condensation of atmospheric air by means of a syringe constructed of glass, in which light is produced, and heat sufficient to inflame tinder submitted to its operation, upon the sudden and forcible depression of the piston.

*Exp.* 4.—For this purpose a strong iron cylinder was employed, bored to the depth of one inch. Into the bore of the cylinder was fitted with tolerable accuracy an iron piston or plug, constructed for the purpose of ramming down and compressing the potassium. A second smaller hole was also drilled partly through the side of the cylinder, for the insertion of the small bulb of a thermometer. A portion of potassium was introduced—the piston was inserted—the apparatus was placed between the extremities of an ordinary vice—and when the thermometer was inserted, and its indications noted down, a considerable compressive force was applied to the extremity of the piston and cylinder. The thermometer ascended in one instance from 53° to 55°, and afterwards by increasing the compression $55\frac{3}{4}$ was obtained. But in most instances not more than $\frac{1}{2}$° to $\frac{3}{4}$° could be obtained with violent compression. By hammering also, the thermometer indicated an increase of heat given off from 55° to $57\frac{1}{2}$°, or $2\frac{1}{2}$° of Fahrenheit; but afterwards this quantity could not be increased by very powerful percussion.

*Exp.* 5.—A strip of pasteboard was now wrapped round the upper part of a new cylinder, and being somewhat elevated above the upper surface of the latter, formed a trough for the reception of mercury surrounding the piston, which would, by the introduction of a thermometer, serve to show the amount of heat given off by the sides of the piston during compression. The apparatus being arranged, the thermometer indicated $63\frac{3}{4}$°. On compression by means of a screw, the thermometer gave 64°, and a second compression 65°; the screw giving way, compression was carried no further.

In another instance the thermometer indicated before compression 66°, and after compression 67°, and no further increase could afterwards be obtained in consequence of the total destruction of the screw.

*Exp.* 6.—On placing the thermometer upon and in contact with the potassium itself, after the removal of the piston, the heat indicated in a few moments was 72°.

## EXTRAORDINARY ISSUE OF LIGHT AND HEAT FROM COMPRESSED POTASSIUM.

*Exp.* 7.—The iron cylinder was again employed, six grains of potassium were introduced, and the whole was placed in a vice for compression. The thermometer indicated 70°. The piston was *depressed* (after being firmly fixed upon the potassium) $\frac{1}{20}$ *of an inch.* To my astonishment, while employed in turning the handle of the vice, *three several and successive explosions occurred,* as loud as that from a small pistol lightly charged, and were accompanied by flame and smoke.

It was discovered that the compressed metal had forced a very minute slit or fissure between its own cell and that in which the thermometer was inserted, and had thus exploded, leaving a stain upon the thermometer (which was unbroken) resembling the marks of exploded gunpowder. On removing the condensed potassium, it was discovered to have the following qualities. It was apparently reduced in bulk, which the depression of the piston indicated, was harder and more friable, and consequently of greater specific gravity, but that I was unable to ascertain.

When placed in its own naphtha after compression, it became somewhat heated; for on placing the bulb of a thermometer in contact with it, the instrument indicated a rise of two degrees, but when removed into the open air in contact with a thermometer, the increase of heat, indicated

in a few seconds, was from 67° to 75°, or 80°. Some augmentation of heat occurred also to a piece of uncondensed potassium when exposed to the atmosphere; but much less rapidly than the former. *The rapidity of combination with oxygen increases in ratio with the increased specific gravity of the metal after compression,* as will be seen throughout these experiments.

## SPONTANEOUS COMBUSTION.

When considerable compression had been employed, the metal was found to absorb oxygen so rapidly as to take fire in a few seconds, and consume with considerable energy until the whole had disappeared. This is not the case at all with the uncompressed metal. During this compressed condition, the metal is seen to enter into fusion upon various parts of its surface. When escaping from an orifice under compression, if the orifice is of large dimensions, it escapes with triflingly increased heat, unchanged, and of a leaden hue, like fine leaden wire or piping.

With a smaller orifice, it is projected unseen, and is discovered shortly afterwards, at some distance in the apartment, by the spontaneous combustion which takes place sooner or later according to the degree of compression employed. With an orifice of a most minute diameter, heat and flame are seen instantly projected, the potassium exploding with considerable illumination and violence.

*Exp.* 8.—An *empty bladder* was now attached to the percussion orifice by means of a short brass tube. I first tested the apparatus, and on striking the piston with a hammer, a considerable explosion ensued twice, attended with the projection of flame and a report as before. When the bladder was screwed to the orifice, and the piston again struck, a flame was seen within the empty bladder, and projecting about two inches, but there was no explosion, and only the sound of the hammer itself.

*Exp.* This experiment was repeated again and again, and still no explosion occurred; but there were noticed, besides the flame, several red-hot pieces of potassium, which fell down to the depending portion of the bladder.

*Exp.* 9.—The bladder was now filled with common air, and the hammer again used for percussion. The application of several smart blows produced no explosion, although the bladder was filled with incandescent potassium, brilliantly luminous even in broad daylight.

The exudation of potassium was now placed beyond doubt; for the finger detected the soapy feel of potash, and the white appearance of this body was observable in every direction throughout the bladder.

It was now discovered that the absence of report was owing to the dilatation of the crevice through which the potassium exuded; for *large* pieces of red-hot metal were projected at every stroke of the hammer.

I very much doubt whether by art I shall be enabled again to witness what by accident has thus been so magnificently thrown in my way.

*Exp.* 10.—By this time a new percussion apparatus was constructed of wrought steel, of considerable strength. The bore was about $\frac{3}{8}$ of an inch, and one inch in depth. The piston was also of steel, and fitted in an air tight manner the bore of the cylinder. A small hole drilled to within $\frac{3}{8}$ of an inch of the former bore, served for the insertion of the thermometer; and the whole, after being charged with a portion of potassium was placed between the extremities of a vice. Considerable pressure was employed; *but in no instance could there be obtained* any certain indication of more *than half a degree* of increased heat given to the cylinder by compression.

Another cylinder was now constructed, also of steel, dimensions as before; but the bore was made to extend

through the entire length of the cylinder. There were two pistons fitted air-tight—one at each end, so as to meet pretty nearly in the centre of the cylinder. At the point of junction a small hole was drilled through the diameter of the cylinder. The first half of this hole was again enlarged and tapped to receive a steel screw, which was made to extend as far as the smaller hole on the opposite side of the bore, and being tapered at its extremity as the screw was turned, entered and closed up the entrance into the smaller orifice, and served as a valve to produce a very minute orifice for exudation or percussion.* The outer bore of this smaller hole was again enlarged externally to $\frac{3}{8}$ of an inch, and tapped to receive the brass neck of the bladder or other apparatus to be employed.

*Exp.* 11.—This new apparatus was now tried, and gave a magnificent flame by percussion, but with considerably less report than before.

*Exp.* 12.—With an attached *bladder filled with oxygen gas*, on percussion a considerable explosion occurred, but instead of bursting the bladder, *it caused its instantaneous contraction to about $\frac{2}{3}$ of its original bulk, and each successive percussion decreased the amount of contained gas until the whole had disappeared.* The bladder was luminous throughout its whole extent, *and the light and heat developed in this instance were more than double the amount* produced by percussion of potassium *in atmospheric air.*

It is thus seen that the cause of *explosion* and report in this instance is *not by the sudden expansion in volume* of gas, as occurs in (I believe) all other cases of explosion, but by the *instantaneous collapse in volume* of an amount of oxygen gas, attracted from the surrounding atmosphere—the report being caused by the rush of air to fill up the vacuity so formed. This is said to be the case in the report caused

* This contrivance was found to fulfil its intention admirably.

by lightning, and here we have a beautiful illustration of the phenomenon.

*Exp.* 13.—A glass tube affixed to a short brass neck by sealing-wax, and bent in the middle to an angle of about 22 degrees, was at its other extremity fitted to the receiver of an air-pump by a cemented leaden tube. The stem was screwed to the percussion orifice. The apparatus being duly arranged upon a vice for percussion, and the tube exhausted to 28 inches—percussion was now made in a room dimly lighted by one small candle. *The ordinary radiating flame was seen issuing from the orifice into the exhausted tube. This flame was considerably inferior in intensity and brilliancy to the ordinary flame developed in atmospheric air, although probably of similar dimensions.*

This experiment proves that the normal light and heat of the potassium flame is not derivable from the atmosphere, or combustion, and exists independently of them.

*Exp.* 14.—The experiment was repeated several times with similar results, the flame extending beyond the elbow or bent portion of the tube, but at all times accompanied with *considerably diminished lustre,* even in the dark. The flame within the tube being retained within certain limits, resembled that which emanates from a blow-pipe of low power; but the fire which issued from the percussion orifice in open air was at all times like that proceeding from the mouth of a pistol, rocket, or other species of fire-work—in diverging lines or radii—and exhibiting the evident *repulsion* of their individual particles.

*Exp.* 15.—On applying a bent copper tube, of about 2 inches in length, to the percussion orifice, no flame could be seen on percussion, probably on account of the conducting and consequently cooling nature of the tube. In one instance, however, large globules of red-hot metal fell from the mouth of the tube.

## AFFINITY OF CALORIC FOR MATTER.

If any one is inclined to doubt whether any affinity really exists between caloric and matter, let him observe the extraordinary effects of condensed carbonic acid; *i. e.*, carbonic acid gas deprived of the caloric which appertains to its gaseous condition. Let him see how mercury is left in the condition of a solid metal by the superior affinity of caloric for the de-calorized gas: how water, by contact with the same, is converted instantaneously into snow or ice—and how the human hand has its texture destroyed by the touch alone—not by chemically caustic powers, but *by the simple demand of this gas for caloric*, which deprives the part of its normal heat before the circulation has time to replenish the same.

The *capacity* of metals appears *to depend upon their relative affinities for caloric;* and it is rather interesting to observe how closely the decimals given by philosophers, as representing their individual capacities, agree with their electro-polar powers as witnessed in "mechanical" and thermo-electricity, subjects which have been brought before this Society, and are published in its *Memoirs*, and also in the *Philosophical Magazine*.

Thus, the capacity of bismuth is stated at ·0288, and it is found to be electro-*negative* to every other metal.

Lead was shown to be electro-*negative* to copper. The capacity of the former is represented by 0·0293, and copper by 0·0949; copper, 0·0949, is electro-negative to iron, ·1100; iron, ·1100, is electro-positive to zinc, ·0927.* Silver, ·0557 —tin, ·0514—gold, ·0298—platinum, ·0314—all take, it will be remembered, a very low place in the scale of amount of current given off.

There appears to be but one exception to this rule,

* This was the case in mechanical and thermo-electricity, although with dilute sulphuric acid zinc is positive to iron. See my former paper.

viz., zinc, ·0927, which is electro-positive to copper, ·0949. In another statement, however, of capacities given by Dr. Ure, it will be found that he names 328·5 for zinc, and only 320, for copper, so that the table in this manner is complete—evincing *the perfect analogy* in numbers of the *capacity for heat and electrical affinities* of each individual metal.

## CONCLUSIONS.

Those who entertain the opinion that heat *is not* electricity, generally advance the argument that in electrical experiments where heat is given off, it is the repulsion of electric force towards caloric which expels the latter or natural calorific force, and causes its diffusion into the surrounding medium.*

To expect to meet with these mutually repulsive forces therefore, as *existing together*, in any large quantity in a given substance, would, according to such an hypothesis, appear absurd.

But the experiments upon potassium detailed before this Society, show that the most intense chemical, electrical, or calorific force may be at any time elicited from this one marvellous substance.

We might state here that the recent experiments of Dr. Faraday leave no doubt as to the fact that flame, smoke, metals, glass, and in fact every kind of matter, are obedient to the laws and influence of magnetism. According to the old notion, therefore, we must have six different kinds of force at all times surrounding the ultimate elements of matter.—But by the hypothesis of their mutual converti-

* If this view were correct, what an extraordinary amount of caloric would be absolutely contained in each substance; for a wire red-hot, and radiating heat between the poles of a Voltaic Battery, would continue red-hot and radiate *for ever*, if the same amount of voltaic force were only constantly maintained!

bility one into another, or as being simply modifications of one universal force, we need but one.

A paper by Professor Draper, has just been published by M. Melloni, and brought forward by him as still further corroborating the analogy of light and heat.

It is highly interesting in this paper to witness the remarkable accuracy with which the development and results of light and heat correspond, by the increase or decrease in temperature of a slip of platinum heated to incandescence by voltaic agency. Professor Draper has *given three columns* in his table, one showing the *degrees of expansion of the platina by the voltaic current*, a second exhibiting the *intensity of light*, and another *that of the heat* given off, as shown by that delicate instrument the thermo-multiplier. *So constant are the three existences in their corresponding developments*, that M. Melloni says "they follow in the progression of quantity the same analogy they do in the progression of quality." Now, it is remarkable that the facts exhibited in two columns should be emphatically noticed, and yet no observation should be made upon the analogy of the third —that the *very circumstances which* to the mind of this great philosopher *evince the perfect identity of light and heat, should be unobserved and unrelated as regards* the results of the first column, *or electricity.*

If the correspondence of the results developed give strong reason to believe that light and heat are analogical, surely the *corresponding* results of the force from which they are developed, declare equally its identity, of which M. Melloni remarks, "the quantities of light emitted by a strip of platina," brought (thus) to different degrees of incandescence, "is the only one by which we could hope for a successful result." *

* After several years of quiet, thoughtful meditation and experiment upon the phenomena of light, heat, electricity, &c., I see no reason to alter

Potassium is thus found to develop a vast amount of caloric, and to evince calorific phenomena of which no other solid substance in nature is capable—and therefore, it is more than probable that its extraordinary chemical and electrical powers are derived from the quantity of absolute caloric which it contains. So far as the analogy of chemical and electrical phenomena is shown by their being the most strongly marked in the same substance, so far equally is it manifested that chemical and electrical phenomena are truly phenomena of heat,—since heat is the existence which at all times so abundantly, and beyond all other forces, predominates in this substance, in which these phenomena are displayed in the highest degree.

Lastly—If it is evidently manifested in my former paper that chemical and electrical phenomena are one and the same thing, because the substance producing the highest chemical affinities develops also the highest electrical phenomena; so now it is equally shown that chemical and electrical forces and caloric *are one* and the same thing, because the substance manifesting the highest chemical and electrical powers, develops also in all its phenomena, both of a mechanical and chemical nature, the highest or most intense quantity of heat.

the opinion given at the meeting of the British Association in 1844, and published in their report :—"That these existences are but the accidental and varied forms (or modifications) of one universal fluid," whose essential qualities in all instances are invariably identical.

## VI.—*On Faults in Farming.* *By* John Just, Esq.

Read May 9, 1848.

The agriculturist stands in a somewhat similar condition, as regards the growth of plants, to that which a contractor for a building does, towards supplying that building with the necessary material. The plans, the construction, the disposition of the parts, the dimensions of the whole, &c., appertain to the architect. Yet unless the contractor fully carries out his engagement, unless he brings to the building a full complement of material, the judicious combinations and skilful adaptations of the several parts cannot be carried into effect, and the labour of the mason is of no avail. So, unless the agriculturist brings to the plants he cultivates, a full complement of what such plants require, to enable them to carry out to the full their capabilities of extension and complete development of all their parts, then are his toil and labour in cultivation to a great extent useless; and what might be a subject of calculation, and almost of certainty (saving the contingencies arising from seasons), is left to risk, and to those natural resources only, which, having long been drawn upon, and in a great measure exhausted, range within narrow limits, and therefore frequently render cultivation unprofitable.

We need not ask a farmer why he cultivates the soil. He does so undoubtedly for a profitable return in produce. All his tillage and application of manure are intended for this purpose. Yet he seldom knows what the complete demands of a growing plant are, so as to render its produce profitable

to him. Experience alone has taught him to expect certain results to follow certain methods of treatment of the soil. Yet experience, such as he possesses, cannot teach him whether the treatment he follows is best calculated to produce such results. He acts without data. He goes on doing so and so, because his ancestors have done the same for many generations before him, and his neighbours still around him continue the practice. As regards any science in agriculture, the common farmer is unfortunately nearly as ignorant as the animals he employs to assist him in his labours. And though he may rise early and toil late, the recompense returned him is inadequate to the pains bestowed, and poverty too often becomes the lot of him, who might, if better instructed, fill his house and the land in which he lives with plenty.

The main object of all the friends of agriculture ought to be to amend this state of things, to improve the farmer's condition. Both by precept and example he ought to be taught. Men of science are in duty bound to come forward, and aid in instructing this hardy and thrifty race of their fellows. Errors should be pointed out, faults exposed, and directions given them respecting an economical and useful expenditure of their labours, and the means of improvement which they have at command. The opulent and the skilful, those who, from having greater advantages, have been better taught, and have carried out in their own practice better management, should encourage them by showing, that the resources of the soil alone, in this country, are incompetent to their maintenance and remuneration; and that on themselves alone, and on the judicious means they employ, and the proper courses they pursue, their prosperity as a body must henceforward depend, as in all circumstances and gradations of society it ought.

Many as are the errors of the common farmers around us,*

* The neighbourhood of Bury is alluded to.

and grievous as they are in themselves, and detrimental to the community at large, no one is more injurious to them than the reckless waste and extravagant expenditure of the manures, and means of increasing tillage, which every where almost they have at their own disposal. Wherever we go we see the manure of the farm-yard carelessly heaped up, and left without thought to all the changes and vicissitudes of the weather. To-day it is baking in the sunshine; to-morrow it is washing away in the rain. Here it stands by a brook, there on the brink of a marl pit, or pool of water, that whatever drains from it may get speedily away. Sometimes a dunghill is placed under the eaves of an outhouse, that it may get the benefit of an extra drenching, to get quit as it were of the impurities it may contain. To judge from this practice we might infer, either that there was no value in the material washed away, or that in itself it was totally useless, or even detrimental, if detained. Yet at certain periods of the year, we observe several of these farmers thus lavish of their farm-yard manure, going to the neighbouring towns, and purchasing various kinds at so much a ton, to make up for the deficiency in the quantity accumulated at home. The conclusion then is, that these farmers must be totally ignorant of the uses of manures in general, or imagine that no diminution in their value and quantity can arise, from the continued action of the air and water to which they are constantly exposed. Before the full extent of such loss as farmers in this way sustain can be ascertained, it is necessary, first, fully to understand the part manures play in the great economy of vegetation.

We have likened the farmer to the contractor for a building, who has to bring together the necessary materials for constructing the same. Manures are known to increase the quantity of produce in the various kinds of crops to which they are applied. They are therefore a material for the crop, or they aid as agents in collecting such material; so

that they have either to be converted directly into the material of the crop itself, or act indirectly to the same extent in collecting it. The mode in which plants act upon such material, and increase their growth by extracting nourishment therefrom, is by drawing it in a fluid or gaseous state, by means of the roots which run within the soil. Roots in general, and particularly those of such plants as are cultivated for agricultural purposes, are extremely and finely branched or ramified, pushing out countless fibrils like gossamer threads, which insinuate themselves between every particle of the soil, and fill every opening or interstice wherein water can filter or air circulate. Each tip of these countless fibrils wants a covering or rind, and consists of numerous minute cellules or sacs, pervious like a sponge to fluid, but wholly destitute of pores, through which any solid particle however minute could introduce itself. Through these tiny sponges the moisture within the soil, with whatever such moisture has dissolved, is being continually drawn into the roots during the plant's growth, along with the fluid air and such other gaseous constituents as may circulate within the soil, or be liberated from sources of decomposition therein. The spongioles deliver what they absorb into finely attenuated textures, and vessels constituting the main mass of the roots; such roots being there protected by a rind or skin, which protects the ascending fluid from any contact with the soil. Herein by their physical properties and vital agencies, the vessels and tissues distribute the fluid throughout the entire plant; such fluid, or as it is now called sap, being acted upon by the various organs through which it passes, until the whole it contains fitted for the growth and structure of the plant has been extracted from it, and secreted and stored up within the interior for the varied purposes of its economy. Each change in the fluid's course induces a movement. The superfluous fluid which conducts such movements, is ultimately thrown off by transpiration

from the leaves and green parts, and the superfluous solid matter either excreted from the same, or thrown out from the root. The organization of a plant thus directs to the organs their own supplies; and the whole mechanism is so contrived as to feed itself, move itself, regulate itself, by inherent powers implanted within it by the great Governor of the universe.

Whatever, then, may be the benefit which plants derive from manures employed in their cultivation, it can only be by such manures entering their structures in a liquid or fluid state. Even the mineral matter certain plants require, such as lime, silica (or the earth of flints), &c., enters the plants in a fluid menstruum. It is hence of no moment to plants how manures, or tillage which acts to their advantage, be applied, the soluble portions alone can be taken up. It is thus that liquid manures, when not overcharged with nutritive matter, act almost immediately in accelerating growth. And solid manures, whatever be their natures, cannot promote growth by other methods or means, than by furnishing plants with their soluble portions, which they distribute throughout the soil. Liquid manures, then, are the most direct and beneficial agents in increasing growth, and should therefore be most carefully preserved and applied. And as solid manures only aid vegetable growth by furnishing a continuous supply of soluble matter, they should be prevented from losing one particle that can be dissolved, until they are spread upon the soil or mingled with it. Tanks and reservoirs for the reception of all liquid and soluble matter from farm-yards, should either be as common as the dunghills, or the dunghills so protected that nothing soluble should flow from them. Such dunghills as must of necessity be exposed to weather, should have their receptacles for drainage and waste, and all others should have the fluid surplus returned them, to keep them in a state of thorough saturation; or, if this be impossible, such

use should be made of the drainage as the nature of the case may require. And, as no growth goes on during the winter months, no kind of liquid manure ought then to be applied to any kind of crop, since that application becomes so diluted by the washings of rains during the inactive period, as to be of little use to growth when its season arrives. Every method, therefore, which ingenuity can devise, ought to be adopted to retain the fluid menstrua during winter; such as the saturation of soils, the carbon of turf, ashes, chaff, refuse of vegetables, or any other absorbent substances, not in themselves detrimental to the soil.

We have already stated, that the farm-yards in this district show no such care and economy. Yet the evil of farm-yard practice ends not here. This one great fault and egregious blunder is not sufficient. After a whole winter's washing such as has been specified, dung must be detained likewise through a part of the summer, to be washed away, as if it required a cleansing before it could be seasonably or profitably applied. And had not nature in this instance, as in many others, been as provident against man's ignorance as she is ready to respond to the calls of his knowledge, in limiting the effects of such washings, and the consequences of decompositions to the surface of dunghills chiefly, the summer spreadings of the winter-collected manures would be nearly a useless expenditure of labour. Much might be said of methods of storing up manures so as to obviate injurious effects, but such is not the object of the present paper; we merely wish to draw attention to the wanton waste of the great staple of agriculture.

We proceed to notice another great fault connected with part of our subject. A common practice is to hoard up a considerable portion of the farm-yard stock of manure for the meadow lands, as tillage for a crop of hay. This portion is reserved, though it has had nothing to do but go through its washings, to the end of July or the beginning

of August, and then is carted out into the meadow which it is intended to benefit for a second crop of hay, soon after the first crop has been gathered. If the weather is moist and favourable, the manure is then spread as a dressing for the crop of the next summer. Another winter it has to lie in the ground, idle. Could ought be conceived more absurd and preposterous? Nearly two years the staple of agriculture doing nothing! Just as wisely in our domestic economy might we boil our meat once, and throw away the broth, then boil the same again, and throw away the broth, and then expect the dry fibres left to contain for our bodies double the nourishment.

Doubtless some reason is assigned for this retention of farm-yard manure during so long a period of time. The reason seems to be, that by spreading the manure in the summer, time is allowed during the following autumn and winter for that manure to get into the ground, and so be ready there for action when the following spring arrives; and, particularly, if such manure has been liberally mixed with straw or vegetable litter, this might become so adherent to the surface of the ground during the lapse of so long a time, that when the following hay harvest arrives, it might retard the scythe in its operations, and rake up along with the hay, to injure the quality of the same. Whereas, if the same manure were spread out during the spring months, it would neither have time to get into the ground before the hay harvest, nor would the straw and litter be connected with the surface of the ground, so as to be out of the way of the scythe and the rake, but would most of it return again to the farm premises, to the no small detriment of the produce. If the action of manure depended upon the condition of its being introduced into the soil, then the first part of such a reason might be admitted. But the fact is, it does not; and therefore there can be no use in such a mode of application. And as to straw and litter, there is no need

of manure with such admixtures to be so expended. Grass crops require no such material; and therefore, whether in the ground or on the ground, there is a loss alike and waste of what the litter contains. Such summer applications may, and they do, make rank crops of eddish; and, if this be the object of such treatment, the end seems in a great degree to be answered. When the subsoil is pervious during the autumnal and winter rains, the greater portion of the soluble parts of the manure gets therein, and the roots of the grasses may follow it, and partake of the beneficial effects; but, in a clayey district like the one around us, such soluble portions are carried off into the drains, and lost for ever. In no case, then, is the practice advisable.

The object of the culture of grass for hay, is to promote to the greatest extent blades, and suppress culms, in order that as much nutriment as possible may be contained in the hay, and as little as possible wasted, by the consumption of the plants themselves in the processes of fecundation and fructification. To secure the greatest quantity of blade possible, highly azotised manures are necessary. Silicated manures make culm more abundant. They are therefore best suited for white crops of grain, and should be used when the land is ploughed: whilst other manures, such as night-soil, guano, liquid manures, dung mixed with soil and de-odorising substances, best suit grass lands, and may be applied most profitably in February, March, and even April of every season, and so be available almost as soon as procurable for an early and profitable return.

This shows that even in the collection of manures in a farm-yard, discretion is necessary. Stable dung, dung from piggeries, &c., must necessarily be mixed up with straw and litter. They ought therefore to be kept separate, and separately applied upon the farm, according to their most suitable services. They should invariably be used on ploughed grounds and for grain crops, for which they are

so calculated to be beneficial; other kinds being reserved for grass lands, green crops, &c. The time, too, for carting out manures from the farm-yard requires thought and management. Frosts oftentimes occur in February, then azotised manures might be applied and spread. Frost and snow can in no way injure manure, though there exists a prejudice against spreading it during their continuance. If they in any way can injure fresh manure on its application, they cannot but in some way, and to a certain extent, injure also that which has been previously spread during the previous summer or autumn.

There is also another fault connected with this part of our subject common in this neighbourhood, and that is, allowing the dung, after being carted out into the meadows, and deposited in heaps, to remain in heaps for an indefinite length of time. Sometimes this is made a matter of convenience. At other times, because the weather is dry it is considered necessary to wait until rain should come before the heaps be spread, lest the drought should injure the manure if spread during its continuance. Both notions are foolish, and alike prejudicial. In the first place, a long continuance of the manure in heaps, rots the roots of the grasses and herbs beneath them, and thence diminishes produce; or, if mere convenience of time be the object of delay, and not weather, then the rains so saturate the soil below the heaps with the soluble portions of the manure, and vegetation becomes so rank, that it lodges before the scythe comes to it, and its quality is injured. Blanched grass grows neither flesh nor blood, milk nor butter. It is from the elaborated juices stored up in the green and growing parts, that animals are supplied with such indispensable elements of their food. Even this ought to be looked to and regarded. Our well-being is in it. Animals are our deputies on the ground, eating night and day to supply us with nourishment so rich, that a few minutes every day may suffice for our maintenance.

Fine and dry weather cannot injure manure. It is true a small amount of ammonia may dissipate from it; but this loss is in no way appreciable. It is merely from the moist fluid parts that ammonia is given off. Dung itself in drying concentrates its ammonia. Fluid matter loses only its watery particles, and hence desiccation injures not the quality of the manure. Were this not the case, where would be the value of guano? Centuries have passed over this manure in a perfectly dry state, and yet it has lost none of its qualities. It acts as freshly and as vigorously as if its application to active soil had taken place when the sea-fowls voided it on the barren beaches of the Peruvian shores. Yet farmers are foolish enough to keep their stocks of guano for several weeks when the weather is dry, for fear, if they should sow it before moisture comes, it should spoil on the ground. We have seen one part of a meadow sown with guano at the beginning of a month of continued drought, produce a crop of grass, not inferior to, but surpassing that of the other part which was sown afterwards when the rain came.

Besides thus separating farm-yard manures, and using them according to their natures and qualities in relation to the crops we are cultivating, we ought never to apply them but when they can immediately act and aid vegetation. Manure, like money, ought never to be idle. Manures can neither lie in nor on the soil doing nothing. Always onward is the course of nature. Whether we profit or not by their decomposition and decay, the process must proceed. If we profit not by the reconstruction of the liberated elements, we lose by them. Hence manures, if both judiciously reserved and judiciously applied, ought to be as judiciously proportioned. Just as much only as will secure one good crop is true economy. Vegetation during the season should lay hold of all we supply. Nothing should be left for waste during winter. Nature keeps a good and equitable balance for all she deals

with. We ought therefore to know how to deal with her, and we shall have justice on our side.

Whatever our crops may be, and whatever we may abstract from the land in produce, it must be our aim always to return equivalents of material in the crude state, for a staple commodity to carry on the business. If we rob the ground by crops of grain, we must carry back to the ground an equivalent of the mineral and other matter we have abstracted, lest our land for cropping become effete. It is owing to the constant robbing of lands, by taking away the grain and the straw without making any sensible or adequate returns, that lands under the plough soon run out, and have to rest till natural processes fill up the drain which mismanagement has made upon them. Hence we make rotations indispensable. Yet did we but treat our grounds skilfully, did we but apply our manures strictly in accordance with their own natures and the requirements of the plants we cultivate, valuable as a rotation system may be, it would not be indispensable; but we might modify our course of agriculture to suit all kinds of contingencies, and take advantage even of unforeseen casualties, by making suitable arrangements so as to make them result to our benefit.

Whenever we want increase of the vegetating organs of plants, we must remember to treat them with manures most abounding in azotised matter. This specially must be studied in our green crops, our clover and Italian ryegrass, our turnips, carrots, cabbages, mangel-wurzel, spring rye, vetches, &c. Not merely is the quantity of the produce raised to a maximum by such treatment, but the quality also improved; a subject worthy of no slight consideration. Among green crops is included the potato, though erroneously so. The produce of the potato is collected in a ripened state, and therefore exhausts the soil to the full amount of what is taken away. Besides, the potato does not require a highly azotised manure. Such manure tends to augment its liabi-

lity to gangrene and caries, which have been so rife for some years past. The potato wants alkaline and carbonaceous elements. Hence the great effect of ashes in which potash abounds, either in a free state or in combination. We may likewise refer the extraordinary produce of the potato on turfy ground, whereon the sward has been burnt for tillage, to the conjoint action of the alkaline and carbonaceous elements. The disease also, though not totally prevented on such grounds, has generally been less virulent there than on others, owing to these favourable conditions.

The wanton waste of the manures in farm-yards, and the faults so common in their application, lie wholly chargeable to the mismanagement of the farmer. There is another quite as bad, however, over which he has no control; and that is, the complete waste of the manures from the sinks and sewerage of our populous towns. The animal returns to the soil all that is unfit to appropriate to its own system. And so does man. The excrement of the former is food again for vegetation, and so is that of the latter. And not only so; but all vegetable refuse and animal waste is equally beneficial with the fæces of either. The drainage from our towns contains immense quantities of all these, which are carried away into the adjoining river as worthless material; the so-called sanitary condition of the towns being urged as a necessity for its entire removal.

Freedom from filth, and a free circulation of the pure air from the country through every avenue of our crowded towns, are essential conditions of health to the communities, and need no argument nor proof. Still, the situation of most of our towns is such as to admit of all the drainage being conveyed in covered conduits to suitable distances, and there amassed in large covered cesspools, for the benefit of agriculture. No injury could arise from such an expedient; no curse, but a blessing. For surely such accumulations of filth thus constantly kept out of contact with the cir-

culating atmosphere, must be less prejudicial than the present system of sewering all the streets of a town to the river, and there allowing the putrid feculent mixture, night and day, summer and winter, to run slowly through the heat of a mighty town, like this of Manchester. If covered sewers be necessary in the one instance, why is not a covering to the river in the other? Is the accumulation of all the evils of a great town, less than that of the individual ones taken separately and singly? This is something like an absurdity, if it be not one entirely.

Yet all this might be a splendid boon to the agriculturist. East, west, north, south, in the direction of the thirty-two points of the compass, Manchester pours out her traffic throughout the adjoining country. From as many points of the compass she imports food into her dense and crowded area. As a return for this food might she not send back her superfluities? From tanks, reservoirs, cesspools, &c., might she not irrigate and enrich the very land she is constantly exhausting? Cheshire is her garden. Thence she obtains most of her green groceries. Cheshire yields her cheese. The refuse returned from articles consumed would manure half the county; and other towns of South Lancashire might so supply the districts which surround them. It would then be idle to talk of guano from Ichaboe or Peru, as we should have at home a sufficient supply in a moist and fresh state for immediate consumption.

Manchester can do any thing which ingenuity and enterprise can compass. And if she will not stir, if she will not raise her giant's strength to this Augean task, the example erelong will be set her. Individual energy somewhere will begin the practice. There is always some one found ready to try a good project. Towns are commonly dependent on the country for produce. In these times of rapid and easy intercourse, let reciprocity prevail, and the country becomes dependent to a certain extent on

the towns for a means of increasing the supply. Distance now-a-days is not worthy calling distance. Twenty miles from a large town is but a suburb, so far as transit and communication are concerned. Let, then, every twenty miles around every town, and along every railway, flourish as the very suburbs of the towns themselves.

# VII.—*Some Remarks on Heat, and the Constitution of Elastic Fluids.* *By* J. P. Joule, F.R.S., &c.

Read October 3, 1848.

In a paper, "On the heat evolved during the electrolysis of water," published in the 7th volume of the Memoirs of this Society, I stated that the magneto-electrical machine enabled us to convert mechanical power into heat; and that I had little doubt that, by interposing an electro-magnetic engine in the circuit of a voltaic battery, a diminution of the quantity of heat evolved, per equivalent of chemical reaction, would be observed, and that this diminution would be proportional to the mechanical power obtained.

The results of experiments in proof of the above proposition were communicated to the British Association for the Advancement of Science, in 1843.* They showed that whenever a current of electricity was generated by a magneto-electrical machine, the quantity of heat evolved by that current had a constant relation to the power required to turn the machine; and, on the other hand, that whenever an engine was worked by a voltaic battery, the power developed was at the expense of the calorific power of the battery for a given consumption of zinc, the mechanical effect produced having a fixed relation to the heat lost in the voltaic circuit.

The obvious conclusion from these experiments was, that heat and mechanical power were convertible into one another; and it became therefore evident, that heat is

* Philosophical Magazine, vol. xxiii. pp. 263, 347, 435.

either the *vis viva* of ponderable particles, or a state of attraction or repulsion capable of generating *vis viva.*

It now became important to ascertain the mechanical equivalent of heat, with as much accuracy as its importance to physical science demanded. For this purpose the magnetic apparatus was not very well adapted; and therefore I sought in the heat generated by the friction of fluids for the means of obtaining exact results. I found, firstly; that the expenditure of a certain amount of mechanical power in the agitation of a given fluid, uniformly produced a certain fixed quantity of heat; and, secondly, that the quantity of heat evolved in the friction of fluids was entirely uninfluenced by the nature of the liquid employed, for water, oil, and mercury, fluids as diverse from one another as could have been well selected, gave sensibly the same result; viz., that the quantity of heat capable of raising the temperature of a lb. of water 1°, is equal to the mechanical power developed by a weight of 770 lbs. in falling through one perpendicular foot.*

Believing that the discovery of the equivalent of heat furnished the means of solving several interesting phenomena, I commenced, in the spring of 1844, some experiments on the changes of temperature occasioned by the rarefaction and compression of atmospheric air.† It had long been known that air, when forcibly compressed, evolves heat; and that, on the contrary, when air is dilated, heat is absorbed. In order to account for these facts, it was assumed that a given weight of air has a smaller capacity for heat when compressed into a small compass than when occupying a larger space. A few experiments served to show the incorrectness of this hypothesis: thus, I found that by forcing 2956 cubic inches of air, at the ordinary

* The equivalent I have since arrived at is 772 foot pounds. See Phil. Trans. 1850, Part I.—May, 1851. J. P. J.

† Philosophical Magazine, vol. xxvi.

atmospheric pressure, into the space of $136\frac{1}{2}$ cubic inches, 13°.63 of heat per lb. of water were produced; whereas by the reverse process, of allowing the compressed air to expand from a stop-cock into the atmosphere, only 4°.09 were absorbed instead of 13°.63, which is the quantity of heat which ought to have been absorbed, according to the generally received hypothesis. I found, also, that when strongly compressed air was allowed to escape into a vacuum, no cooling effect took place on the whole, a fact likewise at variance with the received hypothesis. On the contrary, the theory I ventured to advocate * was in perfect agreement with the phenomena; for the heat evolved by compressing the air was found to be the equivalent of the mechanical power employed, and, *vice versâ,* the heat absorbed in rarefaction was found to be the equivalent of the mechanical power developed, estimated by the weight of the column of atmospheric air displaced. In the case of compressed air expanding into a vacuum, since no mechanical power was produced, no absorption of heat was expected or found. M. Seguin has confirmed the above results in the case of steam.

The above principles lead, indeed, to a more intimate acquaintance with the true theory of the steam-engine; for they have enabled us to estimate the calorific effect of the friction of the steam in passing through the various valves and pipes, as well as that of the piston in rubbing against the sides of the cylinder; and they have also informed us that the steam, while expanding in the cylinder, loses heat in quantity exactly proportional to the mechanical force developed. †

* I subsequently found that M. Mayer had previously advocated a similar hypothesis, without, however, attempting an experimental demonstration of its accuracy.—*Annalen* of Woehler and Liebig for 1842.—May, 1851. J. P. J.

† A complete theory of the motive power of heat has been recently

The experiments on the changes of temperature produced by the rarefaction and condensation of air, give likewise an insight into the constitution of elastic fluids; for they show that the heat of elastic fluids is the mechanical force possessed by them; and since it is known that the temperature of a gas determines its elastic force, it follows that the elastic force, or pressure, must be the effect of the motion of the constituent particles in any gas. This motion may exist in several ways, and still account for the phenomena presented by elastic fluids. Davy, to whom belongs the signal merit of having made the first experiment absolutely demonstrative of the immateriality of heat, enunciated the beautiful hypothesis of a rotary motion. He says, "It seems possible to account for all the phenomena of heat, if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity and through the greatest space: that in fluids and elastic fluids, besides the vibratory motion, which must be considered greatest in the last, the particles have a motion round their own axes with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocity of the vibrations; increase of capacity on the motion being performed in greater space; and the diminution of temperature during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round

communicated by Professor Thomson to the Royal Society of Edinburgh. In this paper the very important law is established, that the fraction of heat converted into power in any perfect engine, is equal to the range of temperature divided by the highest temperature above absolute zero.—May, 1851. J. P. J.

their axes at the moment when the body becomes fluid or aeriform, or from the loss of rapidity of vibration in consequence of the motion of the particles through greater space."* I have myself endeavoured to prove that a rotary motion, such as that described by Sir H. Davy, will account for the law of Boyle and Mariotte, and other phenomena presented by elastic fluids;† nevertheless, since the hypothesis of Herapath, in which it is assumed that the particles of a gas are constantly flying about in every direction with great velocity, the pressure of the gas being owing to the impact of the particles against any surface presented to them, is somewhat simpler, I shall employ it in the following remarks on the constitution of elastic fluids; premising, however, that the hypothesis of a rotary motion accords equally well with the phenomena.

Let us suppose an envelope of the size and shape of a cubic foot to be filled with hydrogen gas, which, at 60° temperature and 30 inches barometrical pressure, will weigh 36·927 grs. Further, let us suppose the above quantity to be divided into three equal and indefinitely small elastic particles, each weighing 12·309 grs.; and further, that each of these particles vibrates between opposite sides of the cube, and maintains an uniform velocity except at the instant of impact; it is required to find the velocity at which each particle must move so as to produce the atmospherical pressure of 14,831,712 grs. on each of the square sides of the cube. In the first place, it is known that if a body moving with the velocity of $32\frac{1}{6}$ feet per second be opposed, during one second, by a pressure equal to its weight, its motion will be stopped, and that, if the pressure be continued one second longer, the particle will acquire

* Elements of Chemical Philosophy, p. 95.

† Mr. Rankine has given a complete mathematical investigation of the action of vortices, in his paper on the Mechanical Action of Gases and Vapours.—Trans. R. S. Edin., vol. xx. part 1.—May, 1851. J. P. J.

the velocity of $32\frac{1}{6}$ feet per second in the contrary direction. At this velocity there will be $32\frac{1}{6}$ collisions of a particle of 12·309 grs. against each side of the cubical vessel in every two seconds of time; and the pressure occasioned thereby will be $12{\cdot}309 \times 32\frac{1}{6} = 395{\cdot}938$ grs. Therefore, since it is manifest that the pressure will be proportional to the square of the velocity of the particles, we shall have for the velocity of the particles requisite to produce the pressure of 14,831,712 grs. on each side of the cubical vessel,

$$v = \sqrt{\left(\frac{14{,}831{,}712}{395{\cdot}938}\right)}\, 32\tfrac{1}{6} = 6225 \text{ feet per second.}$$

The above velocity will be found equal to produce the atmospheric pressure, whether the particles strike each other before they arrive at the sides of the cubical vessel, whether they strike the sides obliquely, and, thirdly, into whatever number of particles the 36·927 grs. of hydrogen are divided.

If only one half the weight of hydrogen, or 18·4635 grs., be enclosed in the cubical vessel, and the velocity of the particles be as before, 6225 feet per second, the pressure will manifestly be only one half of what it was previously; which shows that the law of Boyle and Mariotte flows naturally from the hypothesis.

The velocity above named is that of hydrogen at the temperature of 60°; but we know that the pressure of an elastic fluid at 60°, is to that at 32° as 519 is to 491. Therefore, the velocity of the particles at 60° will be to that at 32° as $\sqrt{519} : \sqrt{491}$; which shows that the velocity at the freezing temperature of water is 6055 feet per second.

In the above calculations it is supposed that the particles of hydrogen have no sensible magnitude, otherwise the velocity corresponding to the same pressure would be lessened.

Since the pressure of a gas increases with its tempera-

ture in arithmetical progression, and since the pressure is proportional to the square of the velocity of the particles, in other words, to their *vis viva,* it follows that the absolute temperature, pressure, and *vis viva* are proportional to one another, and that the zero of temperature is 491° below the freezing-point of water. Further, the absolute heat of the gas, or, in other words, its capacity, will be represented by the whole amount of *vis viva* at a given temperature. The specific heat may, therefore, be determined in the following simple manner:—

The velocity of the particles of hydrogen, at the temperature of 60°, has been stated to be 6225 feet per second, a velocity equivalent to a fall from the perpendicular height of 602,342 feet. The velocity at 61° will be $6225 \sqrt{\frac{520}{519}}$ $= 6230{\cdot}93$ feet per second, which is equivalent to a fall of 603,502 feet. The difference between the above falls is 1160 feet, which is therefore the space through which 1 lb. of pressure must operate upon each lb. of hydrogen, in order to elevate its temperature one degree. But our mechanical equivalent of heat shows that 770 feet is the altitude representing the force required to raise the temperature of water one degree; consequently the specific heat of hydrogen will be $\frac{1160}{770} = 1{\cdot}506$, calling that of water unity.

The specific heats of other gases will be easily deduced from that of hydrogen; for the whole *vis viva* and capacity of equal bulks of the various gases will be equal to one another; and the velocity of the particles will be inversely as the square root of the specific gravity. Hence the specific heat will be inversely proportional to the specific gravity, a law which has been arrived at experimentally by De la Rive and Marcet.

In the following table I have placed the specific heats of various gases determined in the above manner, in juxta-

position with the experimental results of Delaroche and Berard reduced to constant volume.

| | Experimental specific heat. | | Theoretical specific heat. |
|---|---|---|---|
| Hydrogen, - - - - | 2·352 | - | 1·506 |
| Oxygen, - - - - | 0·168 | - | 0·094 |
| Nitrogen - - - - | 0·195 | - | 0·107 |
| Carbonic oxide - - - | 0·158 | - | 0·068 |

The experimental results of Delaroche and Berard are invariably higher than those demanded by the hypothesis. But it must be observed, that the experiments of Delaroche and Berard, though considered the best that have hitherto been made, differ considerably from those of other philosophers. I believe, however, that the investigation undertaken by M. V. Regnault, for the French Government, will embrace the important subject of the capacity of bodies for heat, and that we may shortly expect a new series of determinations of the specific heat of gases, characterized by all the accuracy for which that distinguished philosopher is so justly famous. Till then, perhaps, it will be better to delay any further modifications of the dynamical theory, by which its deductions may be made to correspond more closely with the results of experiment.*

* If we assume that the particles of a gas are resisted uniformly until their motion is stopped, and that then their motion is renewed in the opposite direction, by the continued operation of the same cause, as in the projection upwards and subsequent fall of a heavy body; the maximum velocity of the particles will be to the uniform velocity required by the theory assumed in the text, as the square root of two is to one, and the comparison of the theoretical with the experimental specific heat will be as follows:—

| | Experimental specific heat. | | Theoretical specific heat. |
|---|---|---|---|
| Hydrogen - - - - | 2·352 | - | 3·012 |
| Oxygen - - - - | 0·168 | - | 0·188 |
| Nitrogen - - - - | 0·195 | - | 0·214 |
| Carbonic oxide - - - | 0·158 | - | 0·136 |

I have just learned that the experiments of Regnault on the specific heat of elastic fluids are on the eve of publication, and doubt not that their accuracy will enable us to arrive at a decisive conclusion as to the correctness of the above hypothesis.—June, 1851. J. P. J.

## VIII.—*Description of a Mineral Vein in the Lancashire Coal Field near Skelmersdale.* *By* E. W. BINNEY, Esq.

Read October 30, 1848.

THE county of Lancaster is famous for its invaluable mines of coal, its nearly inexhaustible rocks of useful building stone, and the abundance of good brick clay which it contains; but it is not rich in mines of lead and copper. True, there are the invaluable hematite iron ores of Furness, the clay ironstones of the lower coal measures, which have been worked in ancient times in many places, as near Burnley and elsewhere, the copper mines of Coniston, and the lead mines of Anglezark; but still, with the above exceptions, the county is not remarkable for metallic deposits. Throughout England this is generally the case in coal-fields, except as respects iron, which is so commonly found in them. No doubt, in other parts of the globe many deposits of much later origin than the coal measures are very productive of metals. It is now well known that most metallic veins are found in the vicinity of rocks of igneous origin; so much so, that the latter are generally considered to be intimately connected with the formation of the former.

Although the county of Lancaster possesses few of those dislocations termed dykes, it bears numerous signs of disturbances in the strata of great extent, especially to the north-west and east of Manchester, where there is evidence that the coal measures have been displaced vertically above 3000 feet; but still there is no appearance of what may be strictly termed *altered* rock, or any thing indicating the

action of great heat, the causes which produced the movements having doubtless been deep-seated in the interior of the earth's crust. These dislocations are commonly known by the name of faults, and their direction is nearly always from N. and N.W. to S. and S.E., although there are a few of them from N.N.E. to S.S.W. They generally contain crystals of carbonate of lime, bisulphuret of iron, and in a few instances sulphuret of lead, although the last is rare. Notwithstanding these great disturbances, however, the strata adjoining the faults are seldom found altered by the action of heat, further than such as may have resulted from great mechanical pressure caused by one side of the rock rubbing against the other.

Sulphuret of lead has been obtained from the lower coal measures at Anglezark. Many years ago, a large sum of money was expended in searching for this metal in about the same strata in the Birtle valley near Heywood; and at the present time Mr. Gisborne is working a vein which intersects the lower workable coal series (the Rochdale mines) at Horridge End, near Whaley Bridge. Many of the main joints of rocks, in all parts of the coal field, often have their sides encrusted with crystals of carbonate of lime and bisulphuret of iron, especially in the vicinity of faults. In the rough rock* at Harrock Hill, and in the same rock in Shuttleworth and at other places, are seen thin veins of sulphate of barytes.

In the white sandstone quarry belonging to Mr. Littler at Scotch Row, near St. Helens, where the remarkable fossil trees with stigmariæ roots were found, there is a main joint from S.E. to N.W. filled with beautiful crystals of carbonate of lime and bisulphuret of iron. Other joints of the stone in this quarry afford sulphuret of lead. In this

* For the position of the rough rock and the Rochdale coal series, see paper by the author in vol. i. p. 78 of the *Transactions* of the Manchester Geological Society.

last locality the contents of the joints bear every appearance of having been precipitated from an aqueous solution on the sides of the fissure, there being no traces of dislocation of the strata, nor any alteration in the nature of the rock enclosing them. Crystals of sulphuret of lead, and more rarely also of sulphuret of zinc, are met with in the hollows of nodules of ironstone, and clearly show that they have been separated from the matrix in which they are now imbedded, when it was in a pasty state. Indeed there is now little doubt but that the contents of metallic veins may be classed under three heads; namely—those injected from below into the fissures of a rock; those which have been infiltrated from above into fissures, and those veins and detached crystals which have separated themselves from the mass in which they are now imbedded when it was in a semi-fluid state.

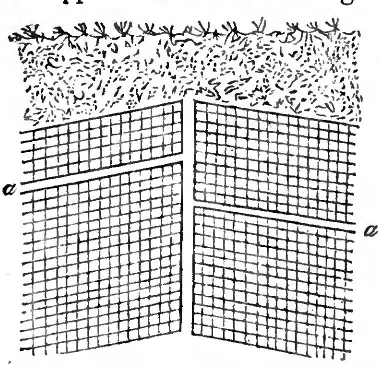
The greater portion of the South Lancashire coal field, west of a line from Manchester to Fenniscowles, near Blackburn, is surrounded on three of its sides by members of the upper new red sandstone formation.* On the southern part of the field, the upper coal measures dip under the sandstone towards the south. On the north side, the millstone grits, on their rise, abut against the sandstone, and dip to the southwest; and on the west, the lower coals do the same, dipping eastwards. On the west side of the coal field are several hills of considerable height, such as Harrock Hill on the north, Parbold Hill, Ashhurst Beacon, and Billinge Beacon, on the west; points which are seen as prominent objects on entering the estuary of the Mersey from the sea. The forces which elevated the coal measures appear to have gone from the moorland district east of Chorley, in a W.S.W. direction to Harrock Hill, and then suddenly turned south to Knowsley and Huyton. On the west side, below Ashhurst Beacon,

* See Map of the Lancashire coal field, by James Heywood, Esq., M.P., F.R.S., published in vol. vi. p. 426 of the Society's *Memoirs*. (New Series.)

is a stone quarry, known by the name of Grimshaw Delph, in which occurs the vein of sulphate of barytes, which it is my intention to describe on the present occasion.

This quarry is situate in a very disturbed district, as is evident from the fact of a rock, having all the characters of the lower new red sandstone, being found on the S.W. side of the Delph, in contact with the rock quarried, which is the rough rock before alluded to, one of the sandstones of the lower coal field. The fault in which the red sandstone lies is the same as that seen in the brook course, below Mr. Stocks' colliery, near Billinge Beacon, running from S.E. to N.W., and thus bringing in the Rainford coal field. The veins of barytes all run at right angles to the direction of this great fault.

The Grimshaw Delph has been quarried for many years, and is of great extent. It consists of a very hard sandstone, composed of sharp grains, identical with that at Parbold; dipping to the N.N.W. at an angle of 16°, and covered by a deposit of six or eight feet of brownish coloured till. The stone is much harder than that of any quarry of the same bed met with in the county, and forms a good material for making and repairing roads. On two occasions, accompanied by his friend Mr. Robert Harkness, the author has had opportunities of examining it.




The main vein or fissure which intersects the quarry, is found on the north-eastern side, where the excavation is about ten yards deep. It is nearly vertical; running from W.S.W., to E.N.E., proceeding from the great N.W. and S.E. fault before alluded to, and having a course nearly at right angles

to it.* The vein varies in thickness from fourteen to twenty inches. It is wider in its upper than in its lower part, and is filled with white crystallized sulphate of barytes, externally discoloured by a little peroxide of iron, and cleaving very freely in a vertical direction. On the walls of the fissure are also seen small crystals of iron pyrites. Enveloped in the barytes are portions of the neighbouring sandstone rock, much altered in structure, and entirely surrounded by it. The vein could not be traced upwards so far as to prove satisfactorily whether or not it went through into the till; but, from the view afforded at the bottom of the quarry, it certainly seems to extend a little higher than the top of the sandstone rock.

The walls of the fissure, although once composed of a laminated sandstone, as is evident from the lines of bedding still seen in some portions of it, are now of a highly crystalline structure, and have an imperfectly developed columnar arrangement of their particles, like basalt. On the north-west of the vein the quarry has been little worked, so that it is not possible to see how far the rock is altered; but on the south-east the whole of the stone in the quarry presents an unusual degree of hardness, and for fifteen yards is very visibly altered. There are several smaller veins of sulphate of barytes traversing the rock, showing little heaves like the large one, and running in a similar direction.† Many of

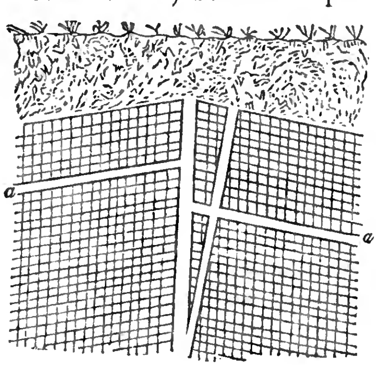
* The veins of barytes at Anglezark, worked many years ago, and described in a paper read before this Society by the late Mr. James Watt, jun., F.R.S., and printed in vol. ii. (1st series) of the Society's *Memoirs*, are of a very similar character to that now described. These also occur chiefly in the rough rock, and proceed from a fault running from S.S.E. to N.N.W., in a direction 60° east of north. They contained sulphurets of lead and zinc, neither of which have been, to the author's knowledge, met with at Grimshaw Delph. All the veins of barytes which he has observed in Lancashire and Yorkshire, have a direction from about N.E. to S.W.

† Since this paper was read the author has again visited the quarry, and

the main joints of the rock, parallel to the fissure, are also traversed by such veins. The chief dip of the stone in the quarry, although now nearly obliterated and difficult to determine, owing to the vertical cleavings of the stone, was found to make an angle of 18° to the north-west. The cleft has evidently been made since the strata were elevated to their present inclination; for on its north side the rock dips to the S.S.E., at an angle of 20°, but on the south side it dips to the N.N.W., at an angle of 11°; thus showing an anticlinal axis differing from the main dip of the quarry.

From the parting of shale in the rock, 4½ inches thick, marked *a* in the woodcuts, there has since been a heave of the strata of about 3 feet 4 inches; this bed, on the north-west side, being that height above the same deposit on the south-east side. Where the vein comes in contact with the bed of shale, the latter has been converted into a stone somewhat resembling greywacke in appearance.

The sandstone rock, from the fragments of coal plants found in it, was no doubt deposited from water; and although it does not now present the laminated structure which is so characteristic of the rough rock series, and which so clearly indicates the mode in which those beds were formed, still it is evident that, before the production of the vein, it was nothing more than a common sandstone. As is usual in all sedimentary deposits, it would have two lines of fracture, one along the lines of deposition, and the other at right angles to those lines. These are known amongst stone masons as the *bed-*




then found that the vein of barytes, on being worked more to the north, showed that it divided into two parts, as described in the above woodcut.

*ding* and *joints* of the stone; the former being generally supposed to be owing partly to the attraction of cohesion of the particles of matter composing the stone, and partly to the presence of silicate of alumina and oxide of iron, which act as a cement; and the latter being produced by the contraction in the bulk of the mass on desiccation—an effect so well seen in clay when it is baked in the sun. This would be the state of the rock long after its first elevation, until some convulsion of the earth connected with the great fault, caused the vertical fissure now occupied by the barytes, and displaced the bed of shale. At that time, either from protrusion in a mass, sublimation, or some other cause in which considerable heat was developed, the barytes was injected into the place where it is now found.

The walls of the vein show every indication of having undergone the action of heat, portions of the sides being split off and mixed with the barytes—an effect which could scarcely have resulted from any other cause than the application of an elevated temperature to the rock. The lines of bedding, and the joints before alluded to in the lower part of the walls, are nearly altogether obliterated, and the sides of the rock cleave vertically. This vertical cleavage is seen in the stone for a considerable distance from the vein; but it is much stronger near the walls than elsewhere, and indicates beyond all question that it has been subjected to great heat, as the same effect is now known to be produced in flag-stones, used as hearths in iron furnaces, many of which have their laminated structure converted into a crystalline structure, by an alteration in the arrangement of the particles composing the stone.

The cleavage of the stone being now vertical, and parallel to the direction of the vein, and not along the joints, indicates that this structure in the mass was caused after it was elevated; or else in all probability the lines would have gone parallel to the joints, which are at right angles to the lines of

deposition. Little up to this time has been done to ascertain the cause of the union of the particles composing sandstone rocks. It has been attempted, as before stated, to be explained partly by cohesive attraction, and partly by the chemical action of silicate of alumina, silicate of lime, and oxide of iron, in the form of a cement; but, from the regularity of the divisional and laminated planes, it is now considered that both the latter are in all probability chiefly due to polar forces.

Dr. Boase, in his treatise on primary geology, at p. 254, cited in p. 281 of Sir H. T. De la Beche's report of the geology of Cornwall and Devon, says, that "in rocks, as in crystals, the integrant particles are combined and arranged into forms more or less geometrical; and that, if the rocks do not exhibit such symmetrical figures as perfect crystals, it may be accounted for by their more complicated composition; so that their forms are not the simple result of the aggregation of similar particles, but the balance of different powers, each tending to produce a different form.". It is not necessary to suppose that the great divisional planes were all formed at one and the same time, but merely to consider that, during the periods when the rocks were consolidating, the matter composing them was brought within the influence of forces tending to divide the masses in directions which slightly deviated from each other when viewed on the large scale, though minor modifications may have been produced by the conditions existing during the consolidation of each rock. Mr. R. W. Fox produced lamination in clay by means of long combined voltaic action; the planes of the laminæ being formed at right angles to the electric forces. He considers that the general laminated structure of the clay in his experiments appeared to indicate, "that a series of voltaic poles were produced throughout the clay, the symmetrical arrangement of which had a corresponding effect on the structure of the clay; and that

this view was confirmed by the occurrence, in several instances, of veins or rather laminæ of oxide of iron, or oxide of copper, according to the manner in which the experiments were conducted."

Some years since, Sir John Herschel, in speaking of cleavage, suggested, that if rocks have been so heated as to allow a commencement of crystallization; that is to say, if they have been heated to a point at which the particles can begin to move amongst themselves, or at least on their own axes, some general law must then determine the position in which these particles will rest on cooling. Probably that position will have some relation to the direction in which the heat escapes. Now, when all, or a majority of particles of the same nature, have a general tendency to one position, that must of course determine the cleavage plane. The above views the author of this paper has seen verified in many instances where laminated shales and sandstones have been thrown out of a coal mine, and deposited without any order one upon another, and, with the small coal mixed with them, set on fire, and subjected to great heat. This mass, after it has been acted upon by sufficient heat, when cooled shows a cleavage sometimes very regular in form, and at other times of a more imperfect structure. But the planes of the cleavage lines are not at right angles to the original planes of deposition, as we should find in baked clay or mud dried in the sun, but at right angles with the cooling surface. Thus laminated shales, which have been placed perfectly level, in an oblique direction, or on their edges, have all the same vertical cleavage, provided the cooling surface be in one and the same direction.

In the case of the vein described in the present communication, the sandstone rock in which it occurs appears to have been elevated to the position of the general dip of the quarry, which is to the N.W., and probably remained in that position for a long period of time. The great S.E.

and N.W. dislocation causing the fissure at right angles to it, and the anticlinal axis was then made. Into this fissure the barytes was injected, and the rock forming the sides subjected to great heat. The mass of the rock having been heated nearly to a state of fusion, would gradually radiate its heat from the upper surface, and thus produce lines of vertical cleavage at right angles to the cooling surface, such as have been previously described.

## IX.—*Remarks on a Vein of Lead found in the Carboniferous Strata in Derbyshire, near Whaley Bridge.* *By* E. W. BINNEY, Esq.

Read February 20, 1849.

IN a paper lately read by me before this Society, a description was given of a vein of barytes at Grimshaw Delph, and allusion was there made to the fact, that mineral veins, containing sulphuret of lead and other metals, had been found in the Great Lancashire coal-field at several places; but that they were not common, and had seldom proved sufficiently rich, so as to render them profitable enough for working. The state of the sandstone rock at Grimshaw Delph appeared to show, that it had been subjected to considerable heat; mention was also made of the circumstances of sulphurets of lead and zinc having been found in druses, or hollows of ironstone nodules, occurring in coal measures, which seemed to indicate that metals had in some instances been precipitated from aqueous solutions, or segregated from semi-fluid masses. As is well known, mineral veins occur in rocks of all ages in different parts of the world. Thus, the zechstein limestone of Germany, the oolitic limestone of Austria, and many other secondary rocks, contain them. With respect to their occurrence in the coal measures, it is to be remarked that they have, in Lancashire and the adjoining districts, been nearly always found in the lower coal field, and generally in or about the position of

the rough rock*—a rock so often confounded with the millstone grits.

The vein which it is intended to describe on the present occasion, was discovered some years since at Horridge End, near Whaley Bridge, by Thomas Gisborne, Esq., in working seams of the lower coals. About a year and a half since, the author had occasion to examine the mine in company with his friend, Mr. David Christie, and he then obtained what information on the subject he possesses.

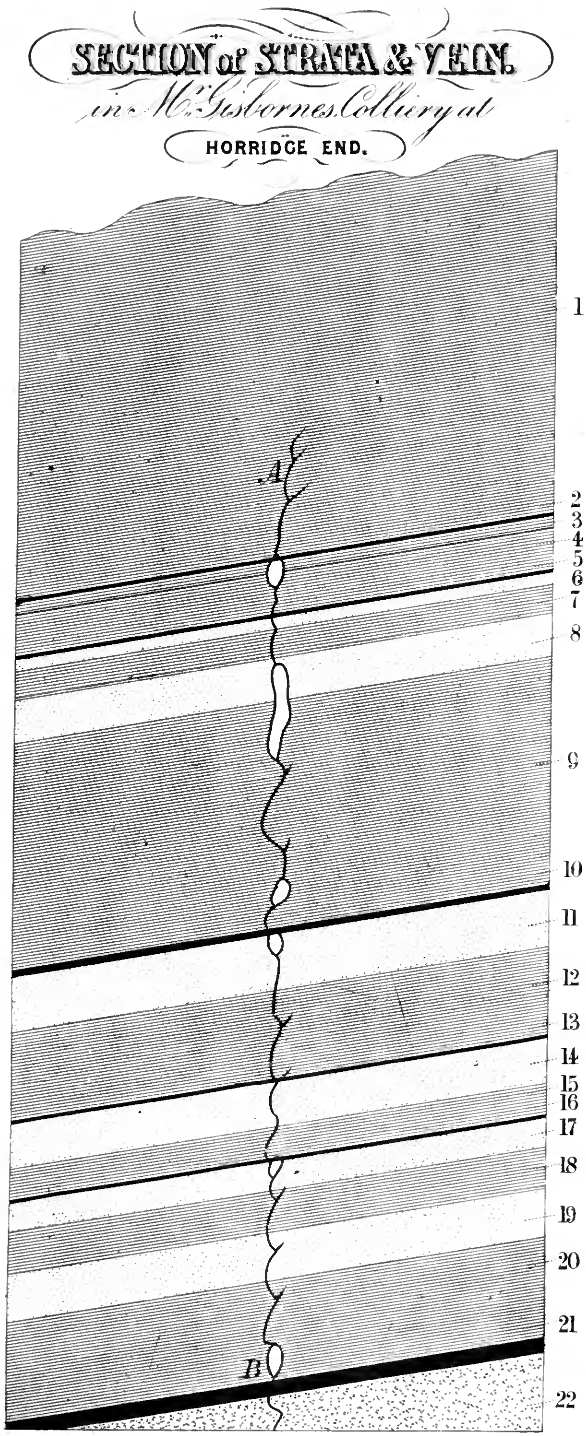
By the kindness of Mr. Gisborne, and his intelligent manager Mr. Sigley, the author is enabled to give the following section:—

| | Yards. | Feet. | Inches |
|---|---|---|---|
| 1 Gray Bind and Shale | 50 | 0 | 0 |
| 2 Coal | 0 | 0 | 9 |
| 3 Fire Clay | 2 | 0 | 0 |
| 4 Gray Bind | 6 | 0 | 0 |
| 5 Thin Coal (a few inches) | | ...... | |
| 6 Stone | 2 | 0 | 0 |
| 7 Gray Bind | 4 | 0 | 0 |
| 8 White Stone | 6 | 0 | 0 |
| 9 Shale | 30 | 0 | 0 |
| 10 Coal | 0 | 1 | 6 |
| 11 Stone | 8 | 0 | 0 |
| 12 Shale | 12 | 0 | 0 |
| 13 Coal | 0 | 1 | 4 |
| 14 Stone | 6 | 0 | 0 |
| 15 Shale | 4 | 0 | 0 |
| 16 Coal | 0 | 0 | 9 |
| 17 Stone | 4 | 0 | 0 |
| 18 Shale | 6 | 0 | 0 |
| 19 Stone | 7 | 0 | 0 |
| 20 Shale | 13 | 0 | 0 |
| 21 Coal | 1 | 1 | 6 |
| 22 Gritstone Rock | | | |

(*See Plate, where the vein is marked by the letter* A.)

The coals above mentioned are those of the Rochdale

* For the position of this rock in the Lancashire coal-field, see p. 78, vol. i., of the *Transactions* of the Manchester Geological Society.


SECTION of STRATA & VEIN.
in Mr. Gisbornes Colliery at
HORRIDGE END.
A
B
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22

series, commencing with the forty yards coal, and terminating downwards with the feather-edge coal lying on the rough rock.

The dip of the coals varies. On the east side of the mine, in the workings of the 18 inches seam, it dips at an angle of 10° to a little north of west; but this dip gradually lessens until the measures become level as you approach the west, when they take an easterly dip. They therefore form a synclinal axis, and are no doubt part of Mr. Farey's Goyt Trough.*

The fissure in which the sulphuret of lead is found, runs about N.E. and S.W.; and one of the miners assured me that it crossed Edale, and then ranged along the country in a N.E. line, so that it probably extends to Balterstone chapel, in Yorkshire.

About 400 yards to the south of Mr. Gisborne's vein is another, running parallel to it. The first-named vein is nearly vertical, but hades a little to the north. There is no heave in the strata showing a displacement, the level of the coal on each side of the seam being the same.

The vein has been proved to extend from the gritstone (rough rock), to within about 20 yards of the surface, and it probably proceeds up to the grass.

The fissure appears to be a simple crack, traversing the different strata, and bears lead about equally in all the beds, whether they are arenaceous or argillaceous. No vein stuff is found with the lead except a little iron pyrites; and the lead appears adhering to the walls of the mine, without either sparry or ochery matter intervening. The vein is often nipped, and varies from nine inches to thin threads in thickness. Where it separates into threads, it traverses the shales and sandstones vertically.

The strata composing the sides of the vein—very unlike

* See vol. i. p. 172, of Farey's *General View of the Agriculture and Minerals of Derbyshire.*

those near the veins of barytes at Grimshaw Delph—show no traces of the action of heat; the joints of the stone and shale near the sides of the vein have a little more vertical cleavage than those rocks have at a distance from it, but that is the only difference which appears. In the 18 inches seam, the lead goes through the coal without in any degree affecting it; for the latter is just as bright and bituminous there as it is in any other part of the seam, and appears to be in no way altered by being in contact with the former. I examined the vein chiefly in the vicinity of the 18 inch coal. This bed appears to me to be identical with the Gannister coal of Rochdale. It has a hard burr floor, which differs in some degree from the usual Gannister, and a black shale roof. In the latter is an abundance of *Avicula papyraceus*, *Goniatites Listeri*, an *Orthoceras*, and a *Posidonia* mixed with many scales of ganoid fishes, amongst which is the *Megalichthys Hibberti*. The walls of the vein, as before stated, were but little altered, and very few portions of them had fallen into the fissure, so generally the case with rake veins.

The main features of this vein of lead are the following; namely, the freedom of the metal from vein stuff, and the bright condition of the coal found in contact with the lead.

The first fact would seem to indicate, that the solution from which the sulphuret of lead was precipitated, had been far purer and freer from other salts than is generally the case with veins of sulphuret of lead, either in carboniferous or limestone districts. The lead found in the coal measures at Anglezark, is enclosed in a mass of sulphate of barytes; and most of the veins of lead in the limestone districts of Derbyshire, are generally enclosed betwixt walls of carbonate of lime, fluor spar, or sulphate of barytes, whilst those found in slate and silurian deposits are generally accompanied by quartz. The veins in red marls and clays,

are for the most filled with sulphate of lime. All these instances appear to show that the vein stuff found in the veins, generally varies with the nature of the rock in which it is found; a circumstance first noticed, I believe, by Sir H. T. De la Beche. In the present instance, the want of the vein stuff seems to show that the metal has not been separated from the rocks which enclose it, but that it has been introduced into the fissure in a state of solution, unmixed with other mineral substances, and then precipitated.

The second fact above alluded to, namely, the bright condition of the coal in contact with the lead, seems clearly to prove that the lead could never have been injected into the fissure in a hot state, or the coal would have been coked and burnt in a similar manner to that which has occurred where a whin dyke has gone through a seam of coal, many instances of which have been met with in the Newcastle and other coal-fields. As previously stated, the miners of the neighbourhood believe that the vein of lead crosses Edale. If this be true, the direction of its course north-eastwards points towards Balterstone chapel, near which place the late Mr. Farey, in vol. i. p. 270 of his survey of Derbyshire, in his list of mines, gives the following: "Wigtwizle, near Balterstone chapel, N.W. of Sheffield, Yorkshire, in grit (perhaps alluvial), lead, black jack, copper." The grit here alluded to is either the rough rock, or one very near in position to it, similar to the lowest grit-stone in the accompanying section. About ten years ago a vein of sulphuret of lead, said to be very rich in silver, was found near to Balterstone, at Deepcar, in lower coal measures; but as it only occurred in detached bunches, similar to Mr. Gisborne's above described, it was not worked to any great extent.

# X.—*Memoir on the Oxides and Nitrates of Lead.* *By* F. Crace Calvert, Esq., *Professor of Chemistry at Manchester.*

Read, December 11, 1849.

## ACTION OF THE CAUSTIC ALKALIES UPON HYDRATE OF PROTOXIDE OF LEAD.

Being requested, when in France, to repeat the interesting experiments of Mr. Fremy, upon the compounds which he called plumbites and plumbates of soda and potash, I did so, and was astonished at the difference of action presented by these two alkalies in the same state of causticity. When put in contact with the hydrate of protoxide of lead, soda dissolved three times as much as potash, a result which I did not expect, as it is generally considered that there is only a slight difference in favour of the former. It is probable that the chemists who considered this point before me, did not act under the same circumstances as myself, and that they employed massicot instead of hydrate of oxide of lead, as offering less chance of error. Therefore, from the molecular state of the oxide not being the same, the action of the alkalies must have been different. To ascertain the real solubility, I took a saturated solution of plumbite of soda or potash at 60°, neutralized it with acetic acid, and passed through the liquor a current of hydro-sulphuric acid. The precipitate thus obtained was washed, and its amount determined.

### CRYSTALLIZED PINK OXIDE—ACTION OF SODA ON THE HYDRATE.

When a solution of boiling caustic soda, of specific gravity 1·454 to 1·375, is saturated with hydrate of oxide of lead and left to cool, two crystallized oxides are deposited; one of them is white, and has been examined by Mr. Payen, the other is pink, and crystallizes in rectangular prisms. This oxide, heated to about 750°, augments in volume, and decrepitates in the same manner as the black oxide of tin, discovered by Mr. Fremy. Whilst this phenomenon is proceeding, a small quantity of water, about one per cent., is given off, owing to a small amount of interposed water, as is remarked in the chloride of sodium and other salts. If the oxide is then left to cool, the intensity of its colour is slightly changed; but if heated to redness and cooled, it becomes yellow, and has the crystalline form presented by the pink oxide. I took advantage of the fact, that this protoxide of lead is slowly dissolved by acids, to separate it from the white, and accordingly treated them by acetic acid, which readily dissolved the white and left the pink, which after being washed was quite pure. Strong or weak nitric acid dissolves this oxide slowly, owing doubtless to its being crystallized and anhydrous. Its specific gravity is from 9·15 to 9·18, and when pulverized it gives a yellow orange powder, similar to that which real massicot produces.

| | | |
|---|---|---|
| 23·445 oxide lost 1·683 of oxygen, or | 7·179 | per cent. |
| Oxide of lead, | 92·821 | „ |
| | 100·000 | |

Its analysis gave me, by taking into account the interposed water, the numbers which represent the protoxide of lead.

### AMORPHOUS RED OXIDE.

Instead of taking the concentrated solution of caustic soda, I melted the solid alkali, and threw into it some hydrate

of protoxide of lead, which became instantly red, and produced a new isomeric protoxide. To take away the excess of alkali, I washed this oxide with spirit of wine and boiled distilled water. This oxide, dried *in vacuo*, has the greatest resemblance to red lead; still it contained no trace of peroxide of lead, as it dissolved readily and entirely in acids; triturated, it gave a powder similar to the pink oxide; heated to above 700 degrees, it became of a darker colour, did not decrepitate, and, on being left to cool, assumed a beautiful sulphur colour.

This oxide retains hygrometric water with the greatest tenacity, therefore great care must be taken in its analysis.

| | | |
|---|---|---|
| 23·213 of oxide lost 1·667 of oxygen, or | 7·18 | per cent |
| Oxide of lead left, | 92·82 | |
| | 100·00 | |

I also made an analysis of the oxide, which became yellow by heat.

| | | |
|---|---|---|
| 12·053 of oxide lost 0·864 of oxygen, or | 7·17 | per cent. |
| Oxide of lead left, | 92·83 | |
| | 100 00 | |

These analyses demonstrate that these two oxides are identical, and that the colour which each of them has is caused by the particular arrangement of their molecules, which, by acting differently on light, causes them to appear to us either red or yellow. This molecular change is due, in my opinion, to the effect of heat, and not to a chemical cause; therefore it appears probable that the colour which massicot takes in becoming red lead, results from the same cause, and is not owing, as is generally believed, to the presence of a small quantity of binoxide of lead, or to the plumbate of protoxide. Moreover, the quantity of this compound in red lead is in too small a proportion to give it its beautiful tint. A fact which substantiates my opinion is, the instantaneous formation of the above amorphous red oxide, when hydrated white oxide is brought in con-

tact with melted caustic soda. In this case there is only the influence of a given temperature, to which the oxide is instantly carried by the heated caustic soda, to account for the production of the red colour. In further proof of the correctness of these views, massicot put into melted caustic soda acquires nearly the same red tint.

I am therefore led to think, that the molecular change which is instantly produced in this experiment, takes place slowly in the oven where massicot is changed into red lead for commercial purposes.

Chemistry already possesses some examples, of a permanent molecular change being effected by the action of heat only. For example, the black phosphorus obtained by Mr. Thenard, and the red amorphous phosphorus lately discovered.

### RED CRYSTALLINE OXIDE—ACTION OF CAUSTIC POTASH ON THE HYDRATED OXIDE OF LEAD.

When a solution of boiling caustic potash is saturated with hydrate of oxide of lead and left to cool, the pink oxide of lead is not produced as with soda, but a beautiful red crystalline one, which was discovered nearly at the same period by Mr. Mitscherlich and myself. This oxide has several of the characters of the pink oxide, but is in smaller crystals, and more soluble in acids.

By throwing into melted caustic potash some hydrate of protoxide of lead, the red amorphous oxide above described is produced.

### CRYSTALLINE OLIVE OXIDE.

To prepare this new oxide, a warm solution of nitrate of lead, having 1·114 specific gravity at 60° Fahrenheit, was poured into a warm solution of concentrated potash, indicating 1·162 specific gravity at 60° Fahrenheit. The subnitrate of lead thus formed, was decomposed by boiling the

whole for one hour, when the olive crystalline oxide was produced. It must be well washed in cold water which has been deprived of its air and carbonic acid by boiling. This oxide, when heated, decrepitates and gives off one per cent. of water. If the heat be then carried to redness, it assumes a beautiful yellow colour, and will not then melt even at a high temperature. It is entirely soluble in acids.

### HYDRATE OF PROTOXIDE OF LEAD.

Requiring for my experiments some pure hydrate of protoxide of lead, I tried the methods known, and was astonished to find, that by whatever means I precipitated nitrate of lead with ammonia, I obtained not the hydrate, but sub-salts of lead. Neither was I more successful when I poured a great excess of potash into nitrate of lead, as I *then* produced sub-nitrates. To prepare this hydrate, a solution of nitrate of lead, of 1·114 specific gravity at 60° Fahrenheit, was poured gently into a solution of caustic potash, having a specific gravity of 1·315°, and the whole boiled for some time. Chemists not agreeing as to the real composition of this hydrate, it was examined, and found to be $= PbO + 2HO$.

### ACTION OF AMMONIA ON THE HYDRATE OF PROTOXIDE OF LEAD.

In enquiring into the action of ammonia on the above hydrate, I was fortunate enough to discover a new compound, which I obtained by taking some hydrate, previously dried between filtering paper, and boiling it for several days in often renewed caustic ammonia. It slowly changes itself into a greenish olive crystalline compound, which, after being separated by decantation from a white powder, well washed in close vessels, and dried *in vacuo*, is the plumbate of ammonia, with one equivalent of water. This compound is crystallized, but in such small crystals as not to be determined; the salt may be kept from decomposition for

an indefinite period, when excluded from the action of the atmosphere. Heated slowly, it gives off water and ammonia; but it is necessary that the heat should be carried to redness ere the whole of the water and ammonia is dissipated. On the application of heat it first assumes a dark red colour, then decrepitates, and leaves when cold a beautiful crystalline yellow massicot. The plumbate of ammonia is easily decomposed by nitric, hydrochloric, sulphuric and acetic acids. Thrown into melted soda, it gives the red oxide. The numbers obtained have led me to assign the following composition to this compound:

| | | Per cent. |
|---|---|---|
| 9·754 of substance | left 9·044 of oxide of lead, or . . . | 92·200 |
| 8·550 „ | gave 2·361 of double chloride,* or 0·563 of ammonia, or . . . . | 6·504 |
| 11·668 „ | „ 0·849 of water and ammonia, or 0·286 of water, or . . . . . | 2·451 |
| | | 101·755 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 3 PbO | = | 333 | = | 92·758 |
| 1 $NH^3$ | = | 17 | = | 4·736 |
| 1 HO | = | 9 | = | 2·506 |
| | | | | 100·000 |

## ACTION OF POTASH ON NITRATE OF LEAD.

By pouring caustic potash into a more or less diluted solution of nitrate of lead, I obtained three salts, two of which were already known, namely, the hydrated, tribasic, and sexbasic nitrates of lead. It is mentioned by authors, that they are prepared by the action of ammonia on the nitrate of lead, in which case I always obtained nitrates containing ammonia. I shall only state, that I prepared the *tribasic nitrate of lead*, by pouring a solution of potash, having a specific gravity 1·076 at 50° Fahrenheit, into a solution of nitrate of lead of a specific gravity 1·375 at 59° Fahrenheit;

* Under the term double chloride, is meant the double chloride of platinum and ammonium.

the *sexbasic nitrate of lead*, by pouring the same solution of potash into a solution of nitrate of lead of specific gravity 1·114 at 60° Fahrenheit. The third salt obtained was an *octobasic nitrate of lead*, which resulted from pouring the same solution of caustic potash into a solution of nitrate of lead, having only a specific gravity of 1·023 at 62° Fahrenheit. This salt is white and amorphous, loses its water when heated slowly, and its nitric acid if heated to redness. The analysis is as follows:—

| | | | | |
|---|---|---|---|---|
| 35·884 of substance | left | 33·260 of oxide, or | | 92·687 |
| 21·608 „ | lost | 1·235 of nitric acid, or | | 5·710 |
| 20·913 „ | „ | 0·386 of water, or | | 1·845 |
| | | | | 100·242 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 8 PbO | = | 888 | = | 92.500 |
| $NO^5$ | = | 54 | = | 5·625 |
| 2 HO | = | 18 | = | 1·875 |
| | | | | 100·000 |

## ACTION OF AMMONIA ON THE NITRATES OF LEAD AT COMMON TEMPERATURES.

The action of these two compounds gave a series of new and interesting salts, of which chemistry possesses no similar example. The action consists in gradually substituting, by ammonia, one of the six equivalents of oxide of lead which exist in the sexbasic nitrate, the other compound of this salt remaining the same. Ammonia and nitrate of lead also afford the means of obtaining tribasic and sexbasic nitrates of lead, analogous in composition to those salts which Sir R. Kean obtained with the nitrates of mercury a few years since, namely, substituting one or two equivalents of their water of crystallisation by one or two equivalents of ammonia.

*The first* salt I shall examine, is derived from the *sexbasic nitrate of lead*, in which ammonia replaces gradually the oxide of lead. I give to these salts the name of *hydrated ammoniacal nitrates of lead.* The first salt I shall describe is the most basic salt of the series, the *hydrated am-*

*moniacal quintibasic nitrate of lead.* When sub-nitrates of lead are boiled in often renewed ammonia, a green powder is formed (which shall be enquired into hereafter), together with a white powder, which from its light specific gravity remains uppermost, and is easily separated by a few decantations. It is then perfectly washed with boiled water, and dried *in vacuo* over sulphuric acid. This white powder, heated slightly, loses its water and ammonia without change of colour; but, if heated to a high temperature, it gradually turns red, sets free its nitric acid, and leaves a beautiful massicot, requiring a great heat for its fusion, a character which is borne by all the massicot hereafter mentioned.

This new salt, although insoluble in water, is readily dissolved in acetic and nitric acids. The analysis of this salt and the following ones, does not present great difficulties; but still, as they have been taken by other chemists for sub-nitrates of lead, and their ammonia overlooked, I think it right to state here, without entering into all the details of analysis, that if a solution of nitrate of lead be mixed with ammonia, the insoluble salts produced invariably contain water and ammonia, which latter is easily detected. These compounds undergo two distinct stages of decomposition;—the first is the production of water and ammonia, and the second is characterised by setting free at a higher temperature, nitric acid.

The hydrated ammoniacal quintibasic nitrate of lead which I have just examined, gave by analysis the following results:—

| | | | Per cent. |
|---|---|---|---|
| 20 836 | of substance | left 18·089 of oxide, or . . . . . . | 86·815 |
| 5·001 | „ | gave 0·417 of nitric acid, or . . . . | 8·338 |
| 26·361 | „ | „ 4·198 of double chloride, or 0·710 of ammonia, or . . . . | 2·693 |
| 5·757 | „ | „ 0·278 of water and ammonia, or 0·125 of water, or . . . | 2.171 |
| | | | 100·017 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 10 PbO | = | 1110 | = | 86·736 |
| 2 $NO^5$ | = | 108 | = | 8·444 |
| 2 $NH^3$ | = | 34 | = | 2·658 |
| 3 HO | = | 27 | = | 2·112 |
| | | | | 100·000 |

Therefore, this new salt is the sexbasic nitrate of lead, in which one equivalent of oxide of lead is replaced by one equivalent of ammonia. The following formula explains its composition :—

Hydrated sexbasic nitrate of lead, 2 ($NO^5$ 6 Pbo) 3 HO

Hydrated ammoniacal quintibasic nitrate of lead, 2($NO^5$ 5PbO, $NH^3$) 3HO.

### ANHYDROUS QUINTIBASIC NITRATE OF LEAD.

This new nitrate is left as a residue by the previous salt, when it is just sufficiently heated to lose its water and ammonia. This salt possesses but little interest. It is a white powder, which becomes of an orange colour when heated, and leaves a beautiful yellow massicot.

These are the numbers furnished by analysis—

| | Per cent. |
|---|---|
| 13·350 of substance lost 1·173 of nitric acid, or | 8·786 |
| Oxide of lead, or | 91·214 |
| | 100·000 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 5 PbO | = | 555 | = | 91·133 |
| 1 $NO^5$ | = | 54 | = | 8·867 |
| | | | | 100·000 |

### HYDRATED AMMONIACAL QUADRIBASIC NITRATE OF LEAD.

This salt was prepared by pouring an excess of ammonia in a stream, into a saturated solution of nitrate of lead, of specific gravity 1·315 at 60° Fahrenheit; and on the whole being left in contact for 24 hours, the precipitate, which seemed amorphous when first thrown down, changed itself gradually into voluminous crystals, which appeared to be oblique rectangular prisms. After washing in a close

vessel with boiled water, and removing every trace of soluble bodies, they were dried *in vacuo* over sulphuric acid. This ammoniacal nitrate of lead, heated at a low temperature, decrepitates, becomes yellow, and loses water and ammonia. If at this period the salt is left to cool, it becomes again white; but if the heat is much increased it acquires a bright red colour, and gives off nitric acid. The crystalline massicot left is of a fine yellow, and is composed as follows:—

| | | | | Per cent. |
|---|---|---|---|---|
| 45·685 | of substance | left | 38·307 of oxide, or . . . . . . . | 83·851 |
| 22·456 | " | lost | 2·315 of nitric acid, or . . . . | 10·309 |
| 22·565 | " | lost | 1·250 of water and ammonia, or 0·524 of water, or . . | 2·322 |
| 18·706 | " | gave | 3·550 of double chloride, or 0·602 of ammonia, or . . . | 3·218 |
| | | | | 99·700 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 8 PbO | = | 888 | = | 84·011 |
| 2 $NO^5$ | = | 108 | = | 10·217 |
| 2 $NH^3$ | = | 34 | = | 3·216 |
| 3 HO | = | 27 | = | 2·556 |
| | | | | 100·000 |

As these numbers shew, this salt is the previous one, in which one equivalent of ammonia has substituted itself for one equivalent of oxide of lead.

Hydrated ammoniacal quintibasic nitrate of lead $2(NO^5\ 5\ PbO, NH^3)3HO$

" " quadribasic " $2(NO^5\ 4\ PbO, NH^3)3HO$

## ANHYDROUS QUADRIBASIC NITRATE OF LEAD.

This compound is derived from the previous salt, and has all the character of a sub-nitrate of lead, therefore it is not necessary to enter into any details. Its analysis is as follows:—

| | |
|---|---|
| 29·325 of substance lost 3·120 of nitric acid, or | 10·632 |
| Oxide left | 89·368 |
| | 100·000 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 4 PbO | = | 444 | = | 89·156 |
| 1 $NO^5$ | = | 54 | = | 10·844 |
| | | | | 100·000 |

HYDRATED AMMONIACAL TRIBASIC NITRATE OF LEAD.

I prepared this salt by pouring an excess of ammonia rapidly into a solution of nitrate of lead, of a specific gravity of 1·080 at 60° Fahrenheit, and leaving the whole in contact during several hours, by which time it had undergone the same molecular change as took place in the previous salt, which, with the exception of being in smaller crystals, it perfectly resembled.

These are the numbers obtained by its analysis :—

| | | | |
|---|---|---|---|
| 24·617 of substance | left | 19·833 of oxide, or . . . . . . | 80·566 |
| 14·971 ,, | lost | 1·945 of nitric acid, or . . . . | 12·991 |
| 15·033 ,, | ,, | 0·957 of water and ammonia, or 0·350 of water, or . . . | 2·328 |
| 24·077 ,, | gave | 5·742 of double chloride, or 0·973 of ammonia, or, . . . . | 4·041 |
| | | | 99·926 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 6 PbO | = | 666 | = | 79·760 |
| 2 $NO^5$ | = | 108 | = | 12·934 |
| 2 $NH^3$ | = | 34 | = | 4·072 |
| 3 HO | = | 27 | = | 3·234 |
| | | | | 100·000 |

The two salts which I am going to examine, are not nitrates of lead in which the oxide is replaced by ammonia, but the tribasic and sexbasic nitrates of lead, in which a given quantity of their water of crystallisation is replaced by an equivalent amount of ammonia.

HYDRO-AMMONIACAL TRIBASIC NITRATE OF LEAD.

This salt is the tribasic nitrate of lead, in which one equivalent of ammonia replaces one of the equivalents of water out of the three which it contains; for example—

| | |
|---|---|
| Hydrated nitrate (tribasic) . . . . | $2\ (NO^5\ 3\ PbO)\ HO\ 2\ HO$ |
| Hydro-ammoniacal nitrate (tribasic) . | $2\ (NO^5\ 3\ PbO)\ NH^3\ 2\ HO$ |

This compound is prepared by pouring caustic ammonia, drop by drop, and not in a stream as in the previous case, into a solution of nitrate of lead, of a specific gravity 1·080 at 60° Fahrenheit; it presents all the characters of the one previously examined.

These are the results of analysis:—

| | | | |
|---|---|---|---|
| 21·160 | of substance left | 17·409 of oxide, or . . . . . . . | 82·277 |
| 21.036 | „ lost | 2·747 of nitric acid, or . . . . | 13·058 |
| 21·145 | „ „ | 0·972 of water and ammonia, or 0·517 of water, or . . | 2·445 |
| 27·210 | „ gave | 3·459 of double chloride, or 0·587 of ammonia, or . . . | 2·157 |
| | | | 99 937 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 6 PbO | = | 666 | = | 82·324 |
| 2 $NO^5$ | = | 108 | = | 13·349 |
| 1 $NH^3$ | = | 17 | = | 2·102 |
| 2 HO | = | 18 | = | 2·225 |
| | | | | 100·000 |

HYDRO-AMMONIACAL SEXBASIC NITRATE OF LEAD.

This new compound is the sexbasic nitrate of lead, in which two of its equivalents of water, of the three which enter into its composition, have been replaced by two of ammonia. It was not prepared by pouring the ammonia into nitrate of lead, but by pouring the latter into the former; and it is also the only one described in this paper which has no crystalline form, and which does not become white again when heated sufficiently to drive off the ammonia and water, but retains a salmon-coloured tint.

Its analysis is as follows:—

| | | | |
|---|---|---|---|
| 36·748 of substance | left 33·013 of oxide, or . . . . . . . | | 89·836 |
| 24·540 „ | lost 1.775 of nitric acid, or . . . . | | 7·233 |
| 17·487 „ | „ 0·556 of water and ammonia, or 0·189 of water, or . . | | 1·080 |
| 28·630 „ | gave 3·565 of double chloride, or 0·602 of ammonia, or . . . | | 2·102 |
| | | | 100.251 |

| Equivalents. | | | | Per cent. |
|---|---|---|---|---|
| 12 PbO | = | 1332 | = | 89·817 |
| 2 $NO^5$ | = | 108 | = | 7·283 |
| 2 $NH^3$ | = | 34 | = | 2·293 |
| HO | = | 9 | = | 0·607 |
| | | | | 100·000 |

I add here a general table of the salts discovered, and those yet necessary to be discovered for completing the series.

| | | |
|---|---|---|
| 1. Type salt . . . . | 2 ($NO^5$ 5 PbO) | + 3 HO |
| 2. New salt . . . . | 2 ($NO^5$ 5 PbO, $NH^3$) | + 3 HO |
| 3. „ . . . . | 2 ($NO^5$ 4 PbO, $NH^3$) | + 3 HO |
| 4. „ . . . . | 2 ($NO^5$ 3 PbO, $NH^3$) | + 3 HO |
| 5. Not yet obtained . . | 2 ($NO^5$ 2 PbO, $NH^3$) | + 3 HO |
| 6. „ . . | 2 ($NO^5$ 1 PbO, $NH^3$) | + 3 HO |

HYDRO-AMMONIACAL NITRATES.

| | | |
|---|---|---|
| 1. Known salt . . . | 2 ($NO^5$ 6 PbO) | + 3 HO |
| 2. New „ . . . | 2 ($NO^5$ 6 PbO) | + 2 $NH^3$ + HO |
| 3. Known salt . . . | 2 ($NO^5$ 3 PbO) | + 3 HO |
| 4. New „ . . . | 2 ($NO^5$ 3 PbO) | + $NH^3$ + 2 HO |

The reasons which induce me to give the formula which I assign to these salts are the following:—First, the uniformity of relation which exists between their different component parts; for, excepting the gradual substitution of one equivalent of oxide of lead by one equivalent of ammonia, their other elements, viz., water and nitric acid, remain unaltered. From these considerations I am led to believe, that as the elements composing the new salts keep the same relation as in the nitrates of lead previously known, I should not make an exception, and represent them as

nitrates of lead in which the ammonia is to be considered as water of crystallisation. If so, they would constitute nitrates of lead with five equivalents of elements of crystallisation. These views are substantiated by the fact, that I obtained two of the known nitrates of lead, in which a certain quantity of their water of crystallisation is replaced by ammonia, and in which the oxide of lead remained in the original proportions.

Secondly. It might be supposed that they were double nitrates of lead and ammonia; but then, heating those salts at a low temperature would not have given water and ammonia, as nitrate of ammonia undergoes by heat a well-known decomposition. And, moreover, it is impossible to admit that the nitric acid of the nitrate of ammonia would have combined itself, when heat was applied, with oxide of lead, and left the ammonia. Before this decomposition could be admitted, it would be requisite to prove that, in the ammoniacal quintibasic nitrate of lead, the nitrate of ammonia is combined with a nitrate of lead containing 10 equivalents of base. One of the most interesting facts contained in this memoir is the influences which the different modes of precipitation have, in more or less concentrated liquors, and in the composition of the bodies which result from them, as exemplified in a striking manner by the preparation of the preceding salts. The only examples which have any similitude to the above are the preparations of the phosphates of lime, which also show how molecular arrangements are changed by slight differences in the *modus operandi*, and which in my opinion are not often looked upon with sufficient care.

## ACTION OF LIQUID AMMONIA IN EBULLITION ON THE PRECEDING SALTS.

When the above salts are boiled for several days with liquid ammonia often renewed, besides the white hydrated

ammoniacal quintibasic nitrate of lead, greenish-yellow crystalline powders are produced, which are separated by the mode above described, well washed in close vessels with spirits of wine, and dried *in vacuo*. Although I prepared these products for many months, and analyzed a great number, still they vary so much in composition that I am in doubt as to their true formulæ. At all events, they present this peculiarity, that even when they have been boiled for weeks with renewed ammonia, well washed and dried over sulphuric acid, they still gave off water, ammonia, *and nitric acid*, the latter of which, most strange to mention, I was unable entirely to remove. I am consequently led to conclude, that the affinity of nitric acid for oxide of lead is nearly equal to its affinity for ammonia. I have little doubt as to the same fact existing with other compounds of lead, as I obtained with the acetate and chloride similar results as with the nitrate. I propose to name these salts *nitrated plumbates of oxide of ammonium*, as they differ entirely from the preceding not only in their greater density, in their colour, in their slow solution in acetic, sulphuric, and nitric acids; but also in giving off ammonia and water when heated, and becoming of a light red. If heated still higher, they decrepitate and give off nitric acid; left to cool, a beautiful crystalline yellow massicot remains.

## CONCLUSIONS.

I. In this memoir are described four new isomeric protoxides of lead, three of which are crystallized—one olive, one red, one pink, and the fourth amorphous and of a fine red. I believe I have demonstrated by this latter, that the colour of red lead has been improperly attributed to a small proportion of binoxide of lead which would exist combined in red lead, and have clearly shown that the colour of red lead is due to a molecular change which massicot undergoes in the oven under the influence of heat.

2. I have succeeded in combining in one proportion the oxide of lead with ammonia, and therefore have produced a plumbate of oxide of ammonium, or plumbate of ammonia, with one equivalent of water. I hope that this new compound will give us the clue to combinations of ammonia with a greater number of oxides than hitherto.

3. I think that most of the directions published for preparing the hydrate of oxide of lead are imperfect, and that the one I give will prove satisfactory.

4. I have succeeded in preparing a new series of salts, in which one equivalent of oxide of lead is gradually replaced by one of ammonia; they therefore present a similitude to the action of chlorine in some organic bodies: that is to say, the ammonia replaces the oxide of lead without changing the harmony which exists in the molecular arrangement of the component parts of the salts.

5. I have also obtained hydrated tri-nitrate and sexbasic nitrates of lead, in which one or two equivalents of the water of crystallization are substituted or replaced by one or two equivalents of ammonia.

6. The above salts show the influence that the degrees of concentration of liquors, and the mode of operating, have on the molecular arrangements of insoluble salts; and I am led to remark, that it is the first instance we have of the influence of different degrees of concentration of solutions on the products which result from their admixture.

7. I have also obtained a new class of compounds, which are interesting, owing to the tenacity with which they retain a small amount of nitric acid.

8. Lastly: I have prepared different sub-nitrates of lead by pouring caustic potash into the nitrate of that metal.

## XI.—*Description of a Meteorite which fell at Allport in Derbyshire. By* ROBERT ANGUS SMITH, PH. D.

Read, December 24, 1849.

*Authentication.*—THE substance to be described was given me by Mr. Benjamin Staly, a gentleman long connected with mining in Derbyshire and Wales, and accustomed to observation in the departments of mineralogy and natural history. He obtained the specimen himself, and preserved it carefully; but he has unfortunately forgotten the exact date, and, having moved about considerably, has not preserved the memoranda concerning it. Somewhere about the year 1827, at the end of August or beginning of September, about three o'clock in the afternoon, a meteor was seen moving along slowly. It was watched by many persons, and was observed to approach the ground near Allport, and burst with a loud report. It fell on a field of mown grass, and fragments were scattered about in all directions. Numerous pieces were picked up by persons in the neighbourhood, and it is rather surprising that there should have been no description given of it. The piece which I received from Mr. Staly, was picked up by him about eighteen hours after he had seen the meteor, and heard the explosion or report.

*Description.*—This meteorite is therefore very well authenticated, although the year and the month are both forgotten; and, even without any such direct testimony, it is a substance which cannot fail to be interesting from its

peculiar appearance and composition. The appearance is that of a piece of common wood charcoal, the lines of structure much crossed, and somewhat resembling the charred knotty part of wood, but too irregular even for that. On taking it into the hand, it is at once perceived to be more than charcoal from its weight, which is 2·08 specific gravity, as nearly as possible. I say as nearly as possible; because, having been weighed both in alcohol and water, a little of the sulphur which it contained may have been removed. (Common wood charcoal may be called at most 1·57 specific gravity, coke 1·8). This weight is not due to the charcoal only, which has a strong resemblance to that of fir, but to the amount of oxide of iron which it contains, amounting to 34·09 per cent.

Spread through the whole mass, there is a sprinkling of sulphur, which might be seen by the microscope to be in a crystalline state. The sulphur amounts to 22·30 per cent.

The principal ingredient in bulk and in weight is charcoal, of which there is 43·59 per cent. I stated in a letter to the Rev. Baden Powell, that it contained no phosphates, sulphurets, or earths. In his report on meteorites, he has given my account of it; but on further examination I have found minute traces of phosphoric acid, of sulphuret of iron, and of lime. I was of course unwilling to destroy entirely the small piece which remained, the description given being sufficient to put it among the most remarkable meteorites; its composition being, as the Rev. Baden Powell remarks in his report to the British Association, 1850, totally unlike that of any other meteorite. The remaining portion of the substance, I have given to be added to the collection of Robert P. Greg, Esq.

I see no clear mode of speculating on its composition. The fine crystals of sulphur seem to shew, that this, like other meteorites, was not heated through the whole mass, but only superficially. This superficial heating gives a glaze

to fallen stones; here there was no material for glaze. The charcoal is a remarkable accompaniment in the form in which it occurs, and seems to throw much suspicion on its origin; but again, the mixture of charcoal with so much oxide of iron and sulphur, combined with the circumstances under which it was found, makes its production in the earth below, almost as difficult to explain as in the heaven above. Mr. Staly says, that the pieces seen by him, and picked up at the moment of explosion, exactly resembled what he himself obtained.

## XII.—*An Experimental Enquiry into the Relative Powers of the Locomotive Engine, and the Resistance of Railway Gradients. By* William Fairbairn, Esq., V.P.

Read February 5, 1850.

A few months previous to the opening of the Liverpool and Manchester railway for general traffic, the locomotive engine was so imperfect in principle as well as construction, as to cause considerable difference of opinion as to whether it should not be entirely dispensed with, and the line worked by horses. This difference of opinion elicited an enquiry into the best mode of working the line; and after a lengthened discussion, it was ultimately resolved, through the determined advocacy of the late Mr. Stephenson, to adopt the locomotive engine.

The Society will recollect with what interest the contest was carried on between the Novelty and the Rocket engines at Rainhill, for the prize which was subsequently awarded by the arbitrators—of which an old and respected member of this Society, Mr. John Kennedy, was the umpire—to Mr. Stephenson. They will also remember how imperfect the construction and how inefficient the engines of those days were to what they are at present. At that period a load of 40 tons, at 10 miles an hour upon a level, was the maximum of their power; and for some years

afterwards it was deemed indispensable to lay out railways as nearly as possible on a level, or with gradients not exceeding 1 in 200 or 1 in 250, for which great sacrifices were made, and large sums of money expended in cuttings, tunnels, and embankments, in order to attain an easy ascent, suitable to the engines then in use.

The first locomotive engines were made respectively of eight, nine, and ten-inch cylinders; but finding them ineffective and deficient in power, adhesion, &c., they were progressively enlarged in all their parts, and the cylinders increased from 10 to 12 inches. Subsequently, the cylinders were still further increased from 12 to 13 and 14 inches, and now they are constructed as high as 16 inches; and in some cases engines of 18-inch cylinders are in use, with all the parts in proportion, weighing from 25 to 30 tons.

This great increase of power rendered the question of gradients, in the construction of new lines of railway, of less importance; and from the increased weight and increased adhesion of the improved engines, gradients, which on former occasions were considered impracticable, are now surmounted without difficulty.

On this point I may instance the Newcastle and Carlisle Railway, where the gradients are about 1 in 106; on the North Union they are 1 in 100; on the Birmingham and Gloucester, 1 in 37; and more recently, in the Edinburgh and Glasgow Tunnel, which is now worked by the locomotive engine, the incline is 1 in 42; and the Manchester and Leeds (Hunt's Bank), from 1 in 46 to 1 in 60. Now, all these gradients can be worked by the locomotive engine as it now exists, with the exception, perhaps, of the Lickey Incline, on the Birmingham and Gloucester Line, which requires two engines; and I have no doubt, as further improvements are effected, and the powers of the

engines still further increased, that gradients of 1 in 30, or probably 1 in 20, may be surmounted.

The means necessary for the working of steep gradients, appear to consist in the power of the engine and the amount of the load which it has to overcome; and, provided the latter is duly apportioned to that of the former at a given velocity, there can be no doubt as to the working of steep gradients with considerable certainty and effect.

The power of the engine required in such cases will vary according to the nature of the traffic; but in every instance where the distance is short and the transit is frequent, light trains may be used and a less powerful engine employed. At other times, where the traffic is less frequent and the transit of heavy trains cannot be dispensed with, it then becomes imperative to employ the most powerful engines, so as to ensure certainty in surmounting the gradients, and that, if possible, without the aid of an assistant engine. Should the gradients, however, be long, and any of them exceed 1 in 60, it may then be necessary under these circumstances to employ an assistant engine as an auxiliary.*

In every case of this kind, the generative power of the engine at the minimum velocity, becomes a question of considerable importance, as steep gradients cannot be efficiently worked excepting under circumstances where a plentiful supply of steam is at hand; and hence arises the necessity of employing engines of greater power, and boilers of more than ordinary capacity, in the area of their tubular surface, than those in general use, accompanied

* Since the above was written, a gradient of nearly two miles in extent has been opened on the East Lancashire Railway, between Accrington and Haslingden, with a rise of 1 in 40. This gradient is worked with one engine to the passenger trains at the rate of nearly 20 miles per hour, and also with ordinary goods trains, excepting only in cases of wet weather and heavy trains, when the pilot engine renders assistance.

with a large fire-box and increased powers of vaporization.

In the discussion of this subject, it may be necessary to enquire into the laws which regulate the different elements of resistance to which the locomotive engine is subjected; and, subsequently, to determine how and in what manner those resistances are to be overcome.

It is well known from practical experience, and also from the experiments of Dr. Lardner, the Comte de Pambour, and Mr. Woods, on Railway Constants, that the resistances are—

1st. The resistance due to friction in the working parts of the engine, and the engine itself considered as a carriage.

2d. The resistance of the carriages, waggons, &c., composing a train. And,

Lastly. The resistance of the air.

In calculating the friction of a locomotive engine, two considerations present themselves; first, the friction of the mechanical organs of the engine considered as a machine; and, secondly, the friction of the engine when considered as a carriage.

From a series of experiments by Pambour these elements are separated; but, taking the friction of the whole engine at 104 lbs., and the average weight at 8 tons, we then have on the datum of 6 lbs. per ton for carriages, a resistance of 56 lbs. for the mechanism of the machinery, and 48 lbs. for the engine when considered as a carriage. Taking, therefore, the united powers of resistance at 13 lbs. per ton as a constant, we cannot be far wrong in estimating the friction of the engine alone at 13 to 15 lbs. per ton, or about two and a half times the friction of a railway waggon.

From the above, it will be observed that the resistance of the motive powers being given, we have next to consider the resistance of a train of carriages and waggons. This retarding force is variously stated by different authors; but

taking the mean of Lardner's, Wood's, and Pambour's experiments, it will be found to approximate to nearly 6 lbs. per ton: and, in the absence of more detailed and more extended experiments, it will not be improper to calculate the forces necessary for ascending steep gradients on the supposition of 6 lbs. per ton being the resisting force per ton of a railway carriage.

Lastly. The resistance of the air, which is again variously estimated.

By Pambour, the resistances are given on every square foot of surface, as follows:—

| | |
|---|---|
| At 20 miles an hour | 1·07 lbs. |
| 22 " " | 1·30 " |
| 24 " " | 1·55 " |
| 26 " " | 1·82 " |
| 28 " " | 2·11 " |
| 30 " " | 2·42 " |
| 32 " " | 2·75 " |
| 34 " " | 3·11 " |
| 36 " " | 3·48 " |
| 38 " " | 3·88 " |
| 40 " " | 4·30 " |
| 42 " " | 4·74 " |
| 44 " " | 5·20 " |
| 46 " " | 5·69 " |
| 48 " " | 6·19 " |
| 50 " " | 6·72 " |

Mr. Woods makes the resistance, on a calm day, at a velocity of 33 miles an hour, equal to $\frac{1}{89}$ of the whole weight, or 25·16 lbs. per ton; and taking 6 lbs. for friction, we then have $25·16 - 6 = 19$ lbs. per ton for the resistance of the air at 33 miles an hour.

In Pambour, we have at 33 miles a resistance of 2·93 lbs. per square foot; and, supposing the area of surface exposed to the action of the atmosphere to be 40 feet, it then follows, that $40 \times 2·93 = 117·2$ lbs., the resistance of the air

against the train; which, in a train of 60 tons, gives a resistance of 19 lbs. per ton. Assuming, therefore, the resistance due to friction, exclusive of the motive powers of the engine, to be 6 lbs., and the resistance of the air 19 lbs., we then have an antagonist force of 25 lbs. per ton in constant operation against the tractive power of a railway train at 33 miles an hour.

From these data, I have endeavoured to calculate the power and size of engines necessary to overcome these resistances (which, it must be borne in mind, are due to a level plain) on different rates of inclination, or on gradients varying from 1 in 20 to 1 in 200. Taking, therefore, 25 lbs. as the measure of resistance on a horizontal plane, the following will exhibit, in a tabular form, the elements of resistance to which a locomotive engine is subjected when ascending gradients varying from 1 in 20 to 1 in 200, at 33 miles an hour:—

TABLE OF RESISTANCES ON RAILWAY GRADIENTS,

| Gradients. | Resistance in lbs. per ton. | Force of resistance due to gravity in lbs. per ton. | Total resistance in lbs. per ton. | Remarks. |
|---|---|---|---|---|
| 1 in 20 | 25 | 112·00 | 137 00 | |
| 1 „ 30 | 25 | 74·66 | 99·66 | |
| 1 „ 40 | 25 | 56·00 | 81·00 | |
| 1 „ 50 | 25 | 44·80 | 69·80 | |
| 1 „ 60 | 25 | 37·33 | 62·33 | |
| 1 „ 70 | 25 | 32·00 | 57 00 | |
| 1 „ 80 | 25 | 28·00 | 53·00 | |
| 1 „ 90 | 25 | 24·88 | 49·88 | |
| 1 „ 100 | 25 | 22·40 | 47·40 | Rate of travelling, 33 miles an hour. |
| 1 „ 110 | 25 | 20·36 | 45·36 | |
| 1 „ 120 | 25 | 18·66 | 43·66 | |
| 1 „ 130 | 25 | 17·23 | 42·23 | |
| 1 „ 140 | 25 | 16·00 | 41·00 | |
| 1 „ 150 | 25 | 14·93 | 39·93 | |
| 1 „ 160 | 25 | 14·00 | 39·00 | |
| 1 „ 170 | 25 | 13·17 | 38·17 | |
| 1 „ 180 | 25 | 12·44 | 37·44 | |
| 1 „ 190 | 25 | 11·78 | 36·78 | |
| 1 „ 200 | 25 | 11·20 | 36·20 | |

Now, if we take the last column of the table, comprising the sum of the total resistance due to the different retarding forces, it will not only be easy to compute the force in lbs., or horses' power necessary to overcome the resistance, but it will also be easy to determine the load which a well-constructed locomotive engine, having 16 inch cylinders, will drag up the differently elevated gradients at the rate of 33 miles an hour.

Before entering upon these calculations, it may however be necessary to state the properties and conditions of the engine on which they are founded; and, in order to ensure sufficient accuracy as regards the power, I have taken the pressure upon the piston at 40 lbs. on the square inch, the cylinders 16 inches diameter, 18 inches stroke, 5 feet driving wheels, and travelling at the rate of 33 miles an hour. Now, an engine of those dimensions, acting with an effective pressure of 40 lbs. on the square inch, will exert a force (the piston moving at a velocity of 555 feet per minute) of 8,924,400 lbs., or 270 horses' power. This taken as a measure of motive power, clearly exhibits the immense force given out by the locomotive engine at high velocities. In the first column of the table we have the inclination of the gradients; in the second, the resistance per ton on a horizontal plane; in the third, the resistance due to gravity; and in the fourth the total resistance per ton, or the retarding force which the engine has to overcome upon every ton raised on gradients varying from 1 in 20 to 1 in 200.*

* On consulting the experiments, it will be found that a less powerful engine (the Baltic), with only 14 inch cylinders, carried a greater load up the Hunt's Bank gradients. This may, however, be accounted for by the increased effective pressure upon the piston, which in this case was from 60 to 65 lbs. on the inch, considerably more than that given above.

## TABLE OF MOTIVE FORCES,

### Applicable to Railway Trains, on Gradients at 33 Miles an Hour.

| Gradients. | Total resistance in lbs. per ton. | Horses' power per ton. | Load in tons for a locomotive engine. |
|---|---|---|---|
| 1 in 20 | 137·00 | 12.05 | 22·4 |
| 1 „ 30 | 99·66 | 8·76 | 30·8 |
| 1 „ 40 | 81·00 | 7·13 | 37·8 |
| 1 „ 50 | 69·80 | 6·15 | 43·9 |
| 1 „ 60 | 62·33 | 5·50 | 49·0 |
| 1 „ 70 | 57·00 | 5·01 | 54·0 |
| 1 „ 80 | 53·00 | 4·66 | 57·9 |
| 1 „ 90 | 49·88 | 4·40 | 61·3 |
| 1 „ 100 | 47·40 | 4·17 | 64·7 |
| 1 „ 110 | 45·36 | 3·98 | 67·8 |
| 1 „ 120 | 43·66 | 3·83 | 70·5 |
| 1 „ 130 | 42·23 | 3·73 | 72·4 |
| 1 „ 140 | 41·00 | 3·58 | 75·4 |
| 1 „ 150 | 39·93 | 3·51 | 76·9 |
| 1 „ 160 | 39·00 | 3·43 | 78·7 |
| 1 „ 170 | 38·17 | 3·36 | 80·3 |
| 1 „ 180 | 37·44 | 3·29 | 82·0 |
| 1 „ 190 | 36·78 | 3·23 | 83·6 |
| 1 „ 200 | 36·20 | 3·18 | 84·9 |
| Level | 25·00 | 2·20 | 122·7 |

From the above results it is obvious that the working of steep gradients is only circumscribed by the power of the engine; and, considering the enormous expense of constructing easy gradients in a mountainous district, it becomes a question of deep interest to the community, in having lines formed at a moderate cost, and that only at the expense of a proportional increase of power. It cannot be doubted that the locomotive engine of the present day is more than commensurate for the attainment of these objects; and, provided we carefully adjust the weight and powers of the engines to the work they have to perform, we may safely calculate on a great saving of expense to the community, increased dividends to the shareholders, and an equally efficient tractive power to overcome the resistances

of retardation in all the elements of gradients varying from 1 in 40 to 1 in 400. As a proof of what can be accomplished in this way, I have to refer to a series of well-conducted experiments, made a few years since on the Hunt's Bank and Halifax inclines, with engines inferior in power, and also of construction, to those now in use. They were probably the best of their kind at that period, but considerably inferior as to weight and power to those which have since been constructed.

The following experiments were undertaken at the request of Mr. Hawkshaw, C.E., and the Directors of the Lancashire and Yorkshire Railway Company, for the purpose of ascertaining whether or not gradients, not exceeding those on the Hunt's Bank incline, could be efficiently worked by the locomotive engine, and whether, and to what extent, improvements could be effected for that purpose. The results are as follows:—

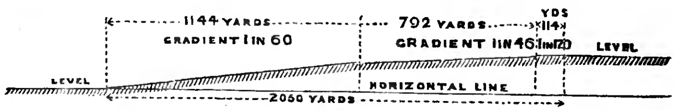
### Experiments made on the Lancashire and Yorkshire Railway, to determine the practicability of working the Hunt's Bank Incline by Locomotive power, instead of the fixed Engines previously erected for that purpose.

*Exp.* 1.—With the locomotive engine, "London," 14 inch cylinders, 20 inches stroke, and 6 wheels, each 4 feet 6 inches diameter, coupled. The load, exclusive of the engine and tender, was composed of 10 waggons, 1 carriage, and 15 passengers.

Weights.

| Wagons. | | | Load. | | | Gross. | |
|---|---|---|---|---|---|---|---|
| Tons. | Cwts. | | Tons. | Cwts. | | Tons. | Cwts. |
| 31 | 12 | ......... | 50 | 10 | ......... | 82 | 2 |
| Of engine and tender | | | | | ...................... | 25 | 13 |
| | | | Total weight | | .............. | 107 | 15 |

With the above load the engine, with steam at 75 to 90 lbs. on the inch, ascended the incline, which varied in the gradients or rates of inclination, as per sketch, as follows:—




In making the ascent, the engine and train were started from nearly the middle of the station; and, having run a distance of about 160 yards, they entered upon the lower gradient of 1 in 60, with a momentum of nearly 14 to 15 miles an hour. Unfortunately, however, the wheels slipped, owing to the moist state of the rails at the entrance of the curve on the gradient of 1 in 46, half way up the incline; the result was a repetition of the experiment.

*Exp.* 2.—The same load in this experiment was carried up the incline, a distance of 2050 yards, in 6 minutes and 4 seconds, being an average rate of travelling of 11·2, or $11\frac{1}{4}$ miles an hour.

*Exp.* 3.—The "Scheldt" engine, 14 inch cylinder, 18 inch stroke, four wheels coupled, 4 feet 8 inches diameter, and two trailing wheels, each 3 feet 6 inches diameter, starting as before, with steam at 65 lbs. on the inch, took the same load, 82 tons 2 cwt., up the incline, a distance of 2050 yards, in 5 minutes and 30 seconds, being at the rate of 12·7 or $12\frac{3}{4}$ miles an hour.

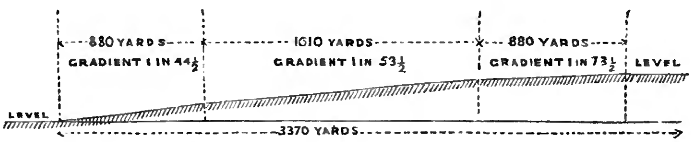
*Exp.* 4.—In this experiment, with the same load and same engine, 6 minutes and 10 seconds were expended in the ascent, owing to the weighing-machine having broken over which the engine and train had to pass, which prevented the train from starting with the same momentum as before.

From these experiments it will be seen, that gradients of considerable elevation can be worked by powerful engines

with heavy trains, at velocities varying in the ratio of the powers of the engines and the loads respectively. A slight drizzling rain was prevalent during the last two experiments, which kept the rails wet, and consequently proved unfavourable for the experiment. The engine, however, retained its full power from the commencement to the top of the incline, without slipping.

The next experiment was on the Halifax incline, which contains three distinct gradients, varying, in a distance of nearly two miles, according to the following longitudinal section.

*Experiment* 5.




With the same engine (the Scheldt) the ascent was accomplished, with a load of 11 tons 1 cwt., exclusive of the engine and tender, in 4 minutes, being at the rate of upwards of 28 miles an hour.

The performance of the Scheldt engine obviously shows that a considerable saving may be effected in the original outlay of great numbers of railways, by the introduction of a class of engines calculated to work the different gradients at a rate of speed corresponding with the nature of the traffic; and, notwithstanding the sacrifice of time, and the increased expenditure of fuel that would have to be made in making the ascent of the gradients, that loss and expenditure would, nevertheless, be compensated, to a considerable extent, by the increased velocity, and consequent saving of coke in the descent.

But in fact, the extra locomotive power which under

such circumstances would be required, is not to be compared to the dead weight of the enormously increased outlay in the first instance, which, in many cases, has been incurred for the purpose of attaining easy gradients, and which, if properly and judiciously applied, would more than supply the motive power in perpetuity for working the whole of the line.

In addition to the experiments on the Hunt's Bank and Halifax inclines, a laborious series of experiments were instituted on the Accrington incline, by Mr. Perring, the talented engineer of the East Lancashire Railway Company; and to that gentleman I am indebted for the following important results.*

These experiments have been conducted with great care, and occupied a period of three months, from the 4th of February 1850 to the 2nd of May, in the worst time of the year.

They become the more interesting, from the circumstance that they are taken from the company's register of the duty performed by each engine, the precise condition under which the ascents of the gradients were made, the weight carried, and the time occupied by each train from the time of starting from the bottom till its arrival at the top of the incline. Another circumstance which renders the experiments valuable, is the fact of them being records of the regular working duty of the engines for three consecutive months, and the great advantages derived from a regular system of working both goods and passenger trains on one of the most difficult inclines in the kingdom. The goods trains, it will be observed, are worked under different cir-

* The Accrington incline is two miles long, having gradients as follow:—

| | | |
|---|---|---|
| Bottom ............ | 1 in 40, 90 chains | = 160 chains, or 2 miles. |
| Middle............. | 1 ,, 38, 48 ,, | |
| Top................ | 1 ,, 47, 22 ,, | |

on a mean incline of 1 in 41·6.

cumstances; the lighter trains with a single engine, and the heavier ones with a double or assistant engine. In the latter case, it is curious to observe the comparatively small power which in many instances was given out by the assistant engine, and the increased quantity of work performed by the single one. In Table I. the single engine ascends the gradients, on an average duty of three months of 38 ascents, with 71·6 tons, at the rate of 6·31 miles an hour; whereas the average duty of the two engines—Table II.—in 10 ascents with 111·9 tons, was at precisely the same speed, or 6·31 miles an hour.

Again, in 15 ascents of the double engines conveying a load of 123·9 tons, the speed was only 5·9 miles per hour, showing an evident saving in the use of the single engine and light trains to a very considerable amount. The results of the experiments are therefore in favour of one engine, which carried 71·6 tons up the incline at the rate of 6·31 miles an hour, whilst the assistant engine carried only 40·3 tons at the same rate. This gives an excess of duty of 31·3 tons in favour of working the gradients by the single engine. In this comparison we must, however, assume the leading engine of the heavy trains to be equally powerful with that of the lighter one. The following tables are however more explicit, and exhibit some curious and important facts in the working of railway gradients.

## Experiments made on the Accrington Incline, with Goods and Passenger Trains, from the 4th February, 1850, to the 2nd of May inclusive.

### TABLE I.

ASCENT OF MERCHANDISE TRAINS UP THE ACCRINGTON INCLINE.

| Particulars of leading Engine. | Particulars of assistant Engine. | Total height of gradients and mean rate of Inclination. | 1850. Dates. | Gross weight of train, exclusive of Engine and Tender, in Tons. | Time occupied in travelling from the bottom to the top of the incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 46 Engine, 18 inch cylinder, 2 feet stroke, six 5 feet wheels, all coupled. Weight of Engine 26.25, Tender 16·75=43 tons. | None. | Distance run, 2 miles on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 4 | 114 | 21 | |
| | | | 16 | 48 | 12 | Rails wet |
| | | | 20 | 60 | 25 | „ slippery. |
| | | | 22 | 84 | 20 | „ slippery. |
| | | | 25 | 90 | 20 | |
| | | | 27 | 18 | 10 | |
| | | | 28 | 72 | 15 | |
| | | | March 1 | 56 | 15 | |
| | | | 5 | 78 | 16 | |
| | | | 7 | 66 | 20 | |
| | | | 8 | 80 | 15 | |
| | | | 9 | 90 | 20 | |
| | | | 11 | 81 | 20 | |
| | | | 12 | 54 | 15 | |
| | | | 13 | 73 | 20 | Rails slippery. |
| | | | 14 | 48 | 15 | |
| | | | 15 | 84 | 18 | Gross weight carried, including Engine and Tender, 43+71·6=114·6 tons. |
| | | | 16 | 48 | 12 | |
| | | | 18 | 49 | 14 | |
| | | | April 6 | 75 | 20 | |
| | | | 8 | 72 | 18 | Rails slippery. |
| | | | 9 | 90 | 23 | „ slippery. |
| | | | 11 | 84 | 25 | |
| | | | 12 | 85 | 20 | |
| | | | 15 | 84 | 23 | Greatest load 114 tons, at 5·71 miles an hour. |
| | | | 16 | 78 | 28 | |
| | | | 17 | 72 | 20 | |
| | | | 19 | 74 | 18 | |
| | | | 21 | 90 | 22 | |
| | | | 22 | 96 | 24 | Average duty performed by the single Engine=71·6 tons up the incline at the rate of 6·31 miles an hour. |
| | | | 23 | 73 | 18 | |
| | | | 24 | 60 | 14 | |
| | | | 25 | 66 | 17 | |
| | | | 28 | 66 | 16 | |
| | | | 29 | 63 | 14 | |
| | | | 30 | 78 | 19 | |
| | | | May 1 | 68 | 18 | |
| | | | 2 | 54 | 15 | |
| | | | Mean. | 71·6 | 18·3 | |

Here the resistance of gravitation upon the mean rise of the three gradients, 1 in 41·6, is 53·87 lbs. per ton. This, added to 25 lbs., which we have already determined as the amount of resistance upon a perfectly horizontal plane,

gives 53·87 + 25 = 78·87 lbs. as the total resistance per ton; or 5645·6 lbs. as the mean tractive force exerted by the engine in raising a load of 71·6 tons up the Accrington incline at the rate of 6·31 miles an hour.

## TABLE II.

### GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assistant Engine. | Total height of gradients, and mean rate of Inclination. | 1850. Dates. | Gross wt. of Train, exclusive of Engine and Tender, in Tons. | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 46 Engine, 18 inch cylinder, 2 feet stroke, six 5 feet wheels, all coupled, weight of Engine, 26·25, Tender 16·75=43 Tons. | No. 12 Engine, 15 inch cylinder, 2 feet stroke, six 4 feet 6 inch wheels, all coupled; weight of Engine 22, Tender 12·50=33½ Tons. | Distance run, 2 miles on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 13 | 108 | 19 | Rails wet. Average duty performed by two Engines = 111·9 tons, at the rate of 6·25 miles an hour. Greatest load 174 tons, at 5·14 mls. an hour. |
| | | | 14 | 144 | 21 | |
| | | | 15 | 96 | 15 | |
| | | | 18 | 93 | 15 | |
| | | | 19 | 120 | 17 | |
| | | | 23 | 96 | 15 | |
| | | | March 2 | 108 | 17 | |
| | | | April 3 | 20 | 20 | Rails wet. |
| | | | 4 | 126 | 25 | |
| | | | 18 | 138 | 28 | |
| | | | Mean. | 111·9 | 19·2 | |
| No. 43 Engine, 18 inch cylinder, 2 feet stroke, six 5 feet wheels all coupled; weight of Engine 25·50, Tender 16·75=42·52 tons. | No. 12 Engine, 15 inch cylinder, 2 feet stroke, six 4 feet 6 inch wheels, all coupled; weight of Engine 22, Tender 12·50=34½ Tons. | | Feb. 13 | 90 | 12 | Rails wet. Average duty performed by two Engines = 123·9 tons, at the rate of 5·9 miles an hour. Greatest load 144 tons, at 4·8 mls. an hour. |
| | | | 14 | 108 | 18 | |
| | | | 15 | 126 | 18 | |
| | | | 16 | 126 | 16 | Rails wet. |
| | | | 18 | 108 | 20 | |
| | | | 19 | 120 | 18 | |
| | | | 20 | 144 | 25 | |
| | | | 22 | 132 | 32 | Rails slipy. |
| | | | 23 | 126 | 18 | |
| | | | 25 | 132 | 20 | |
| | | | March 18 | 96 | 15 | |
| | | | April 3 | 111 | 23 | Rails wet. |
| | | | 4 | 138 | 25 | |
| | | | 6 | 138 | 22 | Rails slipy. |
| | | | 8 | 144 | 25 | |
| | | | Mean. | 123·9 | 20·4 | |
| | No. 24 Engine, 15 in. cylinder, 2 ft. stroke, four 4 feet 9 inch wheels, all coupled; weight of Engine 21, Tender 12½=33½ tons. | | March 5 | 132 | 25 | Average duty performed by two Engines = 133·5 tons, at the rate of 4·25 miles an hour. Greatest load 144 tons, at 4·8 miles an hour. |
| | | | 7 | 138 | 25 | |
| | | | 8 | 144 | 25 | |
| | | | 27 | 120 | 22 | |
| | | | Mean. | 133·5 | 28·7 | |

In the above Table it will be observed, that the rate of travelling to the ratio of the load is somewhat variable, as No. 43 engine, with its assistant, performed an increased rate of duty from February 13th to April 8th, above what it performed from March 5th to the 27th inclusive, where the rate of travelling, in proportion to the load carried, fell off upwards of a mile an hour.

## TABLE III.

### GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of inclination | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of Incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 43 Engine, 18 inch cylinder, 2 feet stroke, six 5 feet wheels, all coupled; weight of engine, 25½, tender, 16¾ = 42¼ tons. | None. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262.8 feet. | Feb. 12 | 72 | 12 | |
| | | | 13 | 60 | 18 | Rails wet. |
| | | | March 2 | 72 | 18 | |
| | | | " | 78 | 19 | |
| | | | 11 | 84 | 25 | |
| | | | April 9 | 72 | 24 | Rails slipppery. |
| | | | " | 78 | 25 | " " |
| | | | " | 60 | 22 | " " |
| | | | 10 | 70 | 20 | |
| | | | " | 66 | 20 | |
| | | | " | 90 | 26 | |
| | | | " | 84 | 23 | |
| | | | 11 | 72 | 23 | Rails slipppery. |
| | | | " | 72 | 24 | " " |
| | | | " | 60 | 19 | " " |
| | | | " | 60 | 18 | " " |
| | | | 12 | 78 | 20 | |
| | | | " | 80 | 22 | |
| | | | " | 66 | 20 | |
| | | | 15 | 66 | 23 | |
| | | | " | 78 | 26 | |
| | | | " | 90 | 28 | |
| | | | 16 | 70 | 20 | |
| | | | 17 | 72 | 23 | |
| | | | " | 72 | 23 | |
| | | | " | 90 | 30 | |
| | | | 18 | 66 | 20 | |
| | | | " | 60 | 18 | |
| | | | " | 65 | 18 | |
| | | | " | 68 | 22 | |
| | | | 19 | 72 | 25 | |
| | | | " | 66 | 20 | |
| | | | " | 67 | 20 | |
| | | | " | 78 | 25 | Average duty performed = 71 tons, at the rate of 5·8 miles an hour. |
| | | | 21 | 54 | 15 | |
| | | | " | 55 | 15 | |
| | | | " | 54 | 14 | |
| | | | 22 | 72 | 20 | |
| | | | " | 66 | 20 | |
| | | | " | 60 | 18 | Greatest load, 90 tons, at 4·81 miles an hour. |
| | | | " | 60 | 19 | |
| | | | 23 | 69 | 25 | |
| | | | " | 66 | 20 | |
| | | | " | 65 | 18 | |
| | | | " | 36 | 12 | |
| | | | 24 | 75 | 23 | |
| | | | " | 84 | 25 | |
| | | | 25 | 78 | 24 | |
| | | | " | 72 | 23 | |
| | | | " | 60 | 20 | |
| | | | 28 | 80 | 23 | |
| | | | " | 74 | 20 | |
| | | | " | 72 | 18 | |
| | | | 29 | 73 | 21 | |
| | | | " | 66 | 19 | |
| | | | " | 72 | 19 | |
| | | | " | 78 | 21 | |
| | | | May 1 | 73 | 18 | |
| | | | " | 72 | 19 | |
| | | | " | 54 | 14 | |
| | | | " | 55 | 15 | |
| | | | 2 | 68 | 20 | |
| | | | " | 66 | 19 | |
| | | | " | 66 | 19 | |
| | | | " | 66 | 20 | |
| | | | Mean ...... | 71 | 20·5 | |

The average load in 65 trips with the single engine is 71 tons, carried up the incline at the rate of 5·8 miles an hour. In these experiments, the duty performed by No. 43 engine is under that of No. 46, which raised the same load at the rate of 6·3 miles to the top of the incline, as

shown in Table I. In other respects they approximate very closely to each other.

## TABLE IV.

### GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradient, and mean rate of inclination | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 43 engine, 18 inch cylinder, 2 feet stroke, six 5 feet wheels, all coupled; weight of engine 25½, tender 16¾ = 42¼ tons. | No. 39 engine, 15 inch cylinder, 2 feet stroke, four 4 feet 9 inch wheels, all coupled, weight of engine 21, tender 12½ = 33½ tons. | Distance run, 2 miles on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | April 9 | 102 | 20 | Rails slippery. |
| | | | 10 | 144 | 20 | |
| | | | 11 | 162 | 30 | " " |
| | | | 12 | 145 | 20 | |
| | | | 15 | 126 | 25 | |
| | | | 16 | 78 | 17 | |
| | | | 17 | 128 | 22 | |
| | | | 18 | 168 | 30 | Average duty performed = 120·8 tons, at the rate of 5·68 miles an hour. Greatest load, 168 tons at 4 miles per hour. |
| | | | 19 | 132 | 20 | |
| | | | 21 | 52 | 15 | |
| | | | 22 | 156 | 25 | |
| | | | 23 | 66 | 15 | |
| | | | 24 | 114 | 20 | |
| | | | 25 | 126 | 25 | |
| | | | 28 | 120 | 19 | |
| | | | 29 | 120 | 18 | |
| | | | May 1 | 134 | 20 | |
| | | | 2 | 132 | 20 | |
| | | | Mean | 120·8 | 21·1 | |
| | No. 45 engine, 16 in. cylinder, 1 ft. 10 in. stroke, six 5 ft wheels, 4 hind coupled, weight of engine 21½, tender 12½ = 34 tons. | | Feb. 28 | 126 | 22 | |
| | | | March 1 | 84 | 12 | |
| | | | 9 | 108 | 20 | Average duty performed = 109·1 tons, at the rate of 5·86 miles an hour. Greatest load, 138 tons, at 4 miles per hour. |
| | | | 12 | 138 | 30 | |
| | | | 13 | 84 | 20 | |
| | | | 14 | 120 | 22 | |
| | | | 15 | 133 | 25 | |
| | | | 16 | 84 | 15 | |
| | | | Mean | 109·1 | 20·7 | |

These experiments present the same uniformity in the force applied by the double engines No. 43 and 39, and also by 43 and 45, as exhibited in the preceding tables. The duty performed is still inferior to that attained by the single engine; but in other respects the performance is remarkably consistent.

## TABLE V.

### GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradient, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 12 Engine, 15 inch cylinder, 2 feet stroke, six 4 feet 6 inch wheels, all coupled; weight of Engine 22, tender 12½ = 34½ tons. | None. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 14 | 51 | 23 | Rails slippery. |
| | | | " | 36 | 15 | " " |
| | | | " | 42 | 18 | " " |
| | | | 15 | 48 | 20 | " wet. |
| | | | " | 48 | 20 | " " |
| | | | " | 42 | 12 | " " |
| | | | " | 42 | 14 | " " |
| | | | 16 | 45 | 20 | |
| | | | " | 48 | 15 | |
| | | | 18 | 60 | 22 | Average duty performed 51·4 tons, at the rate of 6·09 miles per hour. |
| | | | " | 36 | 15 | |
| | | | " | 48 | 15 | |
| | | | 19 | 54 | 20 | |
| | | | " | 48 | 18 | |
| | | | 20 | 49 | 20 | |
| | | | " | 54 | 22 | |
| | | | 22 | 54 | 20 | Rails slippery. |
| | | | " | 54 | 20 | " " |
| | | | " | 54 | 23 | " " |
| | | | 23 | 48 | 18 | |
| | | | " | 54 | 22 | |
| | | | 25 | 42 | 20 | Engine out of order. |
| | | | " | 48 | 22 | " " |
| | | | " | 48 | 23 | " " |
| | | | " | 36 | 17 | |
| | | | " | 42 | 20 | |
| | | | March 2 | 54 | 18 | |
| | | | " | 54 | 17 | |
| | | | 18 | 58 | 20 | |
| | | | " | 66 | 21 | |
| | | | April 3 | 60 | 24 | Rails wet. |
| | | | " | 63 | 25 | " " |
| | | | " | 60 | 20 | " " |
| | | | 4 | 48 | 20 | " " |
| | | | " | 54 | 20 | " " |
| | | | " | 54 | 22 | |
| | | | 6 | 60 | 20 | |
| | | | " | 72 | 25 | |
| | | | 8 | 46 | 19 | Engine out of order. |
| | | | " | 30 | 22 | " " |
| | | | 9 | 60 | 20 | |
| | | | " | 54 | 20 | |
| | | | 30 | 57 | 20 | Greatest load 72 tons, at 5.22 miles an hour. |
| | | | " | 60 | 20 | |
| | | | " | 72 | 23 | |
| | | | Mean | 51.4 | 19.7 | |

Comparing the above experiments with those in Tables I. and III., where the single engine is employed, a considerable diminution of speed is observable to the weight of the load carried. This is accounted for by the reduced powers of the engine, which had 15 instead of 18 inches cylinders, only two-thirds of the large engines. The loads carried in both cases will therefore be found nearly proportional to the powers of the engines and loads respectively.

## TABLE VI.

### GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 39 Engine, 15 inch cylinder, 2 feet stroke, four 4 feet 9 inch wheels, all coupled; weight of Engine 21, tender 12⅓ = 33⅓ tons. | No. 12 Engine, 15 inch cylinder, 2 feet stroke, six 4 feet 6 inch wheels, all coupled; weight of Engine 22, tender 12½ = 34½ tons. | Distance run 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 14 | 120 | 22 | |
| | | | 15 | 114 | 20 | |
| | | | 16 | 84 | 15 | |
| | | | 18 | 102 | 18 | |
| | | | 19 | 120 | 21 | Rails slippery. Av'rage duty performed = 113·6 tons, at the rate of 4·87 miles an hour. Greatest load 138 tons, at 4·61 mls. pr. hour |
| | | | 23 | 108 | 23 | |
| | | | Mar. 2 | 114 | 15 | |
| | | | 18 | 102 | 20 | |
| | | | April 3 | 108 | 25 | " wet. |
| | | | 4 | 120 | 24 | |
| | | | 6 | 138 | 26 | |
| | | | 8 | 115 | 20 | |
| | | | 30 | 132 | 25 | |
| | | | Mean | 113·6 | 24.6 | |
| | No. 45 Engine, 16 in. cylinder, 1 ft. 10 in. stroke; six 5 ft. whls., 4 hind coupled; wt. of Engine 21½, tender 12½ = 34 tons. | | Feb. 28 | 78 | 14 | |
| | | | Mar. 1 | 132 | 30 | |
| | | | 9 | 114 | 22 | |
| | | | 14 | 100 | 20 | Rails slippery. Av'rage duty performed = 107·6 tons, at the rate of 5·7 miles an hour. Greatest load 132 tons, at 4 miles per hour. |
| | | | 15 | 114 | 20 | |
| | | | 16 | 108 | 20 | |
| | | | Mean | 107·6 | 21 | |
| | None. | | Feb. 25 | 78 | 25 | |
| | | | 27 | 60 | 20 | Average duty performed = 55·2 tons, at the rate of 5·7 miles an hour. Greatest load 78 tons, at 4·8 miles per hour. |
| | | | Mar. 3 | 60 | 30 | |
| | | | 15 | 36 | 15 | |
| | | | Feb. 21 | 42 | 16 | |
| | | | | 55·2 | 21·2 | |

In the last five experiments contained in this Table with the single engine, the duty performed approximates closely to that of the double engine, the rate of travelling and the load carried being nearly the same. It will, however, be observed, that the cylinders of the single engine, No. 39, are only 15 inches, whereas the assistant engine in the six previous experiments, No. 45, had cylinders of 16 inches, which reduces the duty of the double engines to nearly the same proportion as that recorded in the previous experiments.

## TABLE VII.

GOODS TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradient, and mean rate of inclination | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 24 Engine, 15 inch cylinder, 2 feet stroke, four 4 ft. 9 in. wheels, all coupled. Weight of engine, 21, tender 12¼ = 33¼ tons. | None. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Mar. 5 | 54 | 20 | |
| | | | „ | 57 | 22 | |
| | | | „ | 54 | 20 | |
| | | | 7 | 42 | 20 | Rails slippery. |
| | | | „ | 48 | 20 | „ „ |
| | | | „ | 48 | 22 | „ „ |
| | | | „ | 54 | 23 | |
| | | | 8 | 46 | 20 | |
| | | | „ | 54 | 20 | |
| | | | Feb. 27 | 54 | 20 | Average duty performed = 52·6 tons, at the rate of 5·5 miles an hour. Greatest load, 57 tons, at 5·45 miles per hour. |
| | | | „ | 48 | 19 | |
| | | | „ | 49 | 19 | |
| | | | Mar. 11 | 54 | 20 | |
| | | | „ | 66 | 25 | |
| | | | „ | 55 | 20 | |
| | | | „ | 60 | 22 | |
| | | | Mean... | 52·6 | 21·7 | |
| No. 45 Engine, 16 inch cylinder, 1 foot 10 inch stroke; six 5 feet wheels, four hind coupled. Weight of engine, 21½, tender, 12½ = 34 tons. | | | Feb. 28 | 36 | 20 | |
| | | | „ | 37 | 23 | |
| | | | „ | 42 | 24 | |
| | | | „ | 42 | 24 | |
| | | | Mar. 1 | 48 | 20 | |
| | | | „ | 48 | 20 | |
| | | | „ | 48 | 19 | |
| | | | 9 | 36 | 18 | |
| | | | „ | 42 | 20 | |
| | | | „ | 42 | 20 | |
| | | | „ | 35 | 18 | |
| | | | 12 | 36 | 15 | |
| | | | „ | 48 | 25 | |
| | | | „ | 42 | 20 | |
| | | | 13 | 38 | 15 | Rls. slippery. Average duty performed=40·8 tons, at the rate of 5·9 miles an hour.—Greatest load, 48 tons, at 6·81 miles per hour. |
| | | | „ | 42 | 20 | „ „ |
| | | | „ | 42 | 20 | „ „ |
| | | | 14 | 40 | 20 | „ „ |
| | | | „ | 36 | 20 | „ „ |
| | | | „ | 36 | 18 | |
| | | | 15 | 42 | 20 | |
| | | | „ | 34 | 18 | |
| | | | „ | 48 | 22 | |
| | | | 16 | 42 | 20 | |
| | | | „ | 30 | 14 | |
| | | | „ | 48 | 22 | |
| | | | April 16 | 42 | 20 | |
| | | | Mean... | 40·8 | 19·8 | |

The same results are indicated in the experiments here recorded as in those already obtained by the single engine. They appear to follow the same law as respects the weight moved, the velocity obtained, and the powers of the engine employed; and, assuming the engines to be in good working order, it will be found that the duty performed by each is nearly in that ratio.

We now come to the Passenger Trains at increased velocities, which indicate some interesting results.

## TABLE VIII.

PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of Inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 36 Engine, 15 inch cylinder, 1 foot 8 inch stroke; six 5 feet 6 inch wheels, 4 hind coupled. Weight of Engine, 18¾, Tender, 12¼ = 31 tons. | No 45 Engine, 16 inch cylinder, 1 foot 10 inch stroke; six 5 feet wheels, 4 hind coupled. Weight of Engine, 21¾, Tender, 12¼ = 34 tons. | Distance run, 2 miles, on a main rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 27 | 25 | 5 | |
| | | | 28 | 25 | 5 | |
| | | | Mar. 9 | 25 | 5 | |
| | | | ,, | 25 | 6 | |
| | | | 11 | 30 | 7 | |
| | | | ,, | 35 | 6 | |
| | | | 12 | 25 | 5 | |
| | | | ,, | 30 | 7 | Average duty performed = 26 tons, at 21·35 miles an hour. Greatest load, 35 tons, at 30 miles per hour. |
| | | | 13 | 20 | 5 | |
| | | | ,, | 25 | 5 | |
| | | | 14 | 25 | 5 | |
| | | | ,, | 25 | 6 | |
| | | | 16 | 25 | 5 | |
| | | | ,, | 26 | 6 | |
| | | | 18 | 20 | 5 | |
| | | | ,, | 30 | 7 | |
| | | | Mean.. | 26 | 5·62 | |

## TABLE IX.

PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of Inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 36 Engine, 15 inch cylinder 1 foot 8 inch stroke; six 5 feet 6 inch wheels, 4 hind coupled. Weight of Engine, 18¾, Tender, 12¼ = 31 tons. | No. 12 Engine, 15 in. cylinder, 2 ft. stroke, six 4 feet 6 inch wheels, all coupled. Wt. of Engine, 22, Tender, 12¼ = 34¼ tons. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 21 | 25 | 7 | R'ls wet |
| | | | ,, | 25 | 7 | ,, |
| | | | 25 | 25 | 6 | Av'rage duty performed = 25·25 tns., at 19·2 mls. an hour. Gr'test load = 26 tns., 24 mls. per hour. |
| | | | ,, | 26 | 5 | |
| | | | Mean . | 25·25 | 6·25 | |
| | No. 14 Engine, 15 in. cylinder, 1 f. 8 in. stroke; six 5 f. wheels, none coupled. Wt. of engine, 17, tender, 11¼ = 28¼ tons. | | Mar. 2 | 25 | 7 | |
| | | | 4 | 21 | 5½ | Average duty performed = 25·16 tons, at 20·3 miles an hour. Greatest load, 30 tons, at 17·14 miles an hour. |
| | | | 5 | 25 | 5 | |
| | | | ,, | 30 | 7 | |
| | | | 15 | 25 | 5 | |
| | | | ,, | 25 | 6 | |
| | | | Mean.. | 25·16 | 59·1 | |
| | None. | | Feb. 22 | 25 | 9 | |
| | | | ,, | 20 | 5 | |
| | | | 27 | 20 | 6 | Average duty performed = 20 tons, at 20 miles an hour. Greatest load, 25 tons, at 20 miles per hour. |
| | | | 28 | 20 | 6 | |
| | | | Mar. 2 | 15 | 5 | |
| | | | 4 | 15 | 5 | |
| | | | 6 | 25 | 6 | |
| | | | Mean.. | 20 | 6 | |

In the transit of passenger trains up long and steep gradients, assistant engines are almost invariably employed. They consist of the regular engines for the conveyance of the train, and an auxiliary engine, which is generally in waiting at the bottom of the incline, to assist by pushing at the tail end of the train till the summit is attained. The mean of the load carried and the speed obtained, in Table IX., with the

assistant engine, is 23·5 tons, at 19·8 miles an hour; whereas, in taking the mean of seven experiments with the leading engine alone, we find that nearly the same weight is raised at about the same velocity, or 20 tons at 20 miles an hour; and, what is still more extraordinary, on March 6th, a load of 25 tons is raised to the top of the incline at the rate of 20 miles an hour by the single engine.

## TABLE X.

### PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of Incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 34 Engine, 15 inch cylinder, 1 foot 8 inch stroke; six 5 feet 6 inch wheels, 4 hind coupled; weight of Engine 18¾, Tender 12¼ = 31 tons. | No. 45 Engine, 16 inch cylinder, 1 foot 10 inch stroke; six 5 feet wheels, 4 hind coupled; weight of Engine 21¾, Tender 12¼, = 34 tons. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Feb. 27 | 35 | 8 | |
| | | | ,, | 25 | 6 | |
| | | | March 9 | 25 | 5 | |
| | | | ,, | 35 | 8 | |
| | | | ,, | 25 | 5 | |
| | | | 11 | 25 | 5 | |
| | | | ,, | 35 | 7 | |
| | | | ,, | 25 | 5 | |
| | | | 12 | 35 | 8 | |
| | | | ,, | 40 | 8 | Average duty performed = 29·13 tons, at 19·44 miles an hour. Greatest load 40 tons, at 13·33 miles per hour. |
| | | | ,, | 25 | 5 | |
| | | | 13 | 32 | 7 | |
| | | | ,, | 36 | 7 | |
| | | | ,, | 26 | 5 | |
| | | | 14 | 25 | 5 | |
| | | | ,, | 30 | 7 | |
| | | | ,, | 25 | 5 | |
| | | | 16 | 26 | 5 | |
| | | | ,, | 30 | 6 | |
| | | | ,, | 25 | 6 | |
| | | | 18 | 25 | 6 | |
| | | | ,, | 35 | 7 | |
| | | | ,, | 25 | 6 | |
| | | | Mean | 29·13 | 6·17 | |
| | No. 12 Engine, 15 in. cylinder, 2 ft. stroke; six 4 ft. 6 in. wheels, all coupled; weight of Engine 22, Tender 12¼ = 34¼ tons. | | Feb. 21 | 15 | 7 | Rails wet. |
| | | | ,, | 35 | 8 | |
| | | | ,, | 25 | 6 | Average duty performed = 31·6 tons, at 16·6 miles an hour. Greatest load 45 tons, at 15 miles per hour. |
| | | | 22 | 38 | 7 | |
| | | | 25 | 45 | 8 | |
| | | | Mean | 31·6 | 7·2 | |

The duty performed respectively by the engines is nearly equal in this Table, and will compare with the trips in Tables VIII. and IX., which, taken in the aggregate, indicate nearly the same results, with the exception only of the performance of the single engine in Table IX., which, as before noticed, greatly exceeds the duty of the double engines.

## TABLE XI.

### PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradients, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 34 Engine, 15 inch cylinder, 1 foot 8 inch stroke; six 5 feet 6 inch wheels, 4 hind coupled; weight of Engine 18¾, Tender 12¼ = 31 tons. | No. 14 Engine, 15 inch cylinder, 1 foot 8 inch stroke; six 5 feet wheels, none coupled; weight of Engine 17, Tender 11¼ = 28¼ tons. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Mar. 2 | 20 | 6 | Average duty performed = 32·08 tons, at 17·14 miles an hour. Greatest load 50 tons, at 13·33 miles per hour. |
| | | | ,, | 35 | 8 | |
| | | | ,, | 45 | 9 | |
| | | | 4 | 20 | 5 | |
| | | | ,, | 50 | 9 | |
| | | | ,, | 25 | 6 | |
| | | | 5 | 30 | 7 | |
| | | | ,, | 45 | 10 | |
| | | | ,, | 30 | 6 | |
| | | | 15 | 25 | 6 | |
| | | | ,, | 35 | 7 | |
| | | | ,, | 25 | 5 | |
| | | | Mean | 32·08 | 7 | |
| | None. | | Feb. 22 | 15 | 7 | Average duty performed = 20·8 tons, at 23 miles an hour. Greatest load 40 tons, at 17·14 miles per hour. |
| | | | ,, | 20 | 5 | |
| | | | 23 | 15 | 4 | |
| | | | 25 | 15 | 6 | |
| | | | ,, | 20 | 5 | |
| | | | 27 | 15 | 5 | |
| | | | 28 | 15 | 5 | |
| | | | ,, | 40 | 7 | |
| | | | ,, | 15 | 5 | |
| | | | Mar. 6 | 20 | 5 | |
| | | | ,, | 35 | 8 | |
| | | | ,, | 25 | 5 | |
| | | | Mean | 20·8 | 5·58 | |

The anomalous condition of the performance of the single and double engines presents a difficulty not easily accounted for. From the 22nd of February to the 6th of March inclusive, the single engine conveyed to the top of the incline twelve trains, with a mean load of 20·8 tons, at 23 miles an hour, and the greatest weight carried on the 28th February, was 40 tons at the rate of 17·14 miles per hour, a higher rate than in any of the former experiments by the single engine. With the same engine, and an assistant of equal power, the greatest load raised is only

50 tons, at the rate of 13·33 miles per hour, a greatly inferior duty to that obtained by the leading engine itself on the 28th of February.

The only reason for this discrepancy would be the imperfect state of the assistant engine, which, in this and the former Tables, appears to have given out a very small proportion of the power, probably little more than sufficient to carry its own weight up the incline. On the other hand, the leading engine must have generated steam rapidly, and, as usually happens in surmounting gradients, at a high pressure, which at once accounts for the great difference which exists in the duty performed by the single and double engines respectively.

## TABLE XII.

### PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradient, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 42 Engine, 15 inch cylinder, 1 foot 8 inch stroke; six 5 feet 6 inch wheels, 4 hind coupled. Weight of Engine, 18⅕, Tender, 12⅘ = 31 tons. | No. 45 Engine, 16 inch cylinder, 1 foot 10 inch stroke; six 5 feet wheels, 4 hind coupled. Weight of Engine, 21¼, Tender, 12¾ = 34 tons. | Distance run, 2 miles, on a mean rise of 1 in 41.6, or a vertical height of 262·8 feet. | Feb. 27 | 20 | 5 | Average duty performed = 28·3 tons, at 18·4 miles an hour. Greatest load, 40 tons, at 13·33 miles per hour. |
| | | | ,, | 36 | 9 | |
| | | | 28 | 20 | 5 | |
| | | | Mar. 1 | 25 | 4½ | |
| | | | 9 | 20 | 5 | |
| | | | ,, | 35 | 7 | |
| | | | 11 | 25 | 6 | |
| | | | ,, | 40 | 9 | |
| | | | 12 | 25 | 6 | |
| | | | ,, | 42 | 9 | |
| | | | 13 | 25 | 6 | |
| | | | ,, | 36 | 8 | |
| | | | 14 | 20 | 5 | |
| | | | ,, | 30 | 8 | |
| | | | 16 | 20 | 5 | |
| | | | ,, | 30 | 6 | |
| | | | 18 | 26 | 6 | |
| | | | ,, | 35 | 8 | |
| | | | | 28·3 | 6·5 | |
| | No. 12 Engine 15 in. cylinder, 2ft. stroke; six [illegible] ft. 6 in. wheels, all coupled. Wgt. of Engine, 22, Tender, 12¼ = 34¼ tons. | | Feb. 11 | 20 | 4½ | Average duty performed = 28·57 tons, at 20·76 miles an hour. Greatest load carried, 40 tons, at 17·14 miles per hour. |
| | | | 21 | 15 | 6 | |
| | | | ,, | 30 | 6 | |
| | | | 22 | 25 | 5 | |
| | | | ,, | 35 | 6 | |
| | | | 25 | 40 | 7 | |
| | | | 27 | 35 | 6 | |
| | | | | 28·57 | 5·8 | |

In the above experiments we have a still further exemplification of the inefficacy of the assisting engine, as the ratio of the load carried to the speed attained is remarkably consistent whenever the assistant engine is employed; and this is the more strikingly apparent as the greatest load, 40 tons, raised in February 25th, is only equal to that drawn by the single engine recorded in the preceding Tables, the speed being reduced to its equivalent of the load.

## TABLE XIII.

### PASSENGER TRAINS.

| Particulars of leading Engine. | Particulars of assisting Engine. | Total height of gradient, and mean rate of inclination. | Dates. 1850. | Gross wt. of Train, exclusive of Engine and Tender, in tons | Time occupied in travelling from the bottom to the top of incline. | Remarks. |
|---|---|---|---|---|---|---|
| No. 42 Engine, 15 in. cylinder, 1 ft. 8 in. stroke; six 5 feet 6 inch wheels, 4 hind coupled. Wt. of Engine, $18\frac{1}{2}$, Tender, $12\frac{1}{3}$ = 31 tons. | No. 14 Engine, 15 inch cylinder, 1 ft. 8 in. stroke; six 5 feet wheels, none coupled. Wt. of Engine, 17, Tender, $11\frac{1}{4}$ = $28\frac{1}{4}$ tons. | Distance run, 2 miles, on a mean rise of 1 in 41·6, or a vertical height of 262·8 feet. | Mar. 2 | 20 | 6 | |
| | | | ,, | 25 | 6 | |
| | | | 4 | 20 | 5 | |
| | | | ,, | 40 | 9 | Average duty performed = 29·4, at 16·85 miles an hour. Greatest load, 45 tons, at 10·9 miles per hour. |
| | | | 5 | 30 | 7 | |
| | | | ,, | 45 | 11 | |
| | | | 15 | 25 | 6 | |
| | | | ,, | 30 | 7 | |
| | | | Mean .. | 29·4 | 7·12 | |
| | None. | | Feb. 25 | 20 | 7 | |
| | | | 27 | 30 | 7 | Average duty performed = 29 tons, at 15 miles an hour. Greatest load, 35 tons, at 13·33 miles per hour. |
| | | | Mar. 6 | 25 | 6 | |
| | | | ,, | 35 | 9 | |
| | | | Feb. 4 | 35 | 11 | |
| | | | Mean .. | 29·0 | 8·0 | |

The mean duty performed by the double engines in the first experiments, is similar to that attained by the leading and assistant engines in the conveyance of the trains at different periods of time. They are all of them in their united capacity defective; and again, computing the work done with the power employed in the above experiments, it is evidently in favour of the single engine. Twenty-nine tons, it will be observed, were conveyed to the top of the incline at the rate of 15 miles an hour by the single engine;

whereas two engines of equal power raised the same weight, or nearly so, at the rate of 16·85 miles per hour, which gives an increase of speed of only 1·85, little more than 1¾ miles an hour. These facts are confirmatory of the advantages of working trains on steep gradients by the single engine, whenever that can be accomplished, with light loads at a moderate rate of speed.

Having effected the arrangement and classification of the experiments, it now becomes necessary to collect them in such form as will enable us to arrive at correct results, and to offer such remarks as may prove useful in determining in what situations, and under what circumstances, the various gradients on railways can be surmounted by the single engine, and when the assistant engine can be applied with advantage. To effect these objects the following summary of results, first, of the goods, and then of the passenger traffic, will be found sufficiently explicit to enable the engineer to judge as to the amount of power required, and whether or not the assistant engine would be conducive to the interests of proprietors and the service of the public.

SUMMARY OF RESULTS,

*As obtained from 270 Experimental Trips in the transit of Goods Trains on the Accrington Incline.*

| Single Engine. | | | | | Double Engine. | | | |
|---|---|---|---|---|---|---|---|---|
| No. of Trips. | Mean weight carried in Tons. | Rate of travelling in miles pr hour. | Power of Engine represented by the area of the cylinders. | | No. of Trips. | Mean weight carried in Tons. | Rate of travelling in miles pr hour. | Power of Engines represented by area of cylinders. |
| 38 | 71·6 | 6·31 | 490·8 | | 10 | 111·9 | 6·25 | 844·2 |
| 65 | 71·0 | 6·80 | 490·8 | | 15 | 123·9 | 5·90 | 844·2 |
| 45 | 51·4 | 6·09 | 353·4 | | 4 | 133·5 | 4·25 | 844·2 |
| 5 | 55·2 | 4·80 | 353·4 | | 18 | 120·8 | 5·68 | 844·2 |
| 16 | 52·6 | 5·50 | 353·4 | | 8 | 109·1 | 5·86 | 892·8 |
| 27 | 40·8 | 5·90 | 402·0 | | 13 | 113·6 | 4·80 | 706·8 |
| | | | | | 6 | 107·6 | 5·70 | 755·4 |
| ... | 57·1 | 5·73 | 427·2 | Mean. | ... | 117·2 | 5·49 | 818·8 |

In the above summary, it will be observed, that assuming the area of the cylinders of the engines employed to be

the measure of the force exerted at a given velocity, and supposing the power of the single engine to be represented by 427·2, we thus have a load of 57·1 tons carried up the different gradients of the incline at the rate of 5·73 miles per hour. This duty, when compared with the double engine, whose representative of force is 818·8, is rather more than twice that of the single engine, a load of 117·2 tons being transmitted over the same gradients at the rate of 5·49 miles an hour. From these results we must infer, that in the transit of heavy trains at a low rate of speed, the advantages are not much in favour of the single engine; yet in cases of light trains, at a higher rate of speed, the benefits arising from the use of the single engine are sufficiently evident to secure it the preference, in every instance where the load is duly and properly regulated to the power of the engine. These facts are, however, more clearly developed in the experiments with the passenger trains, as follows:—

SUMMARY OF RESULTS,

*As obtained from 123 Experimental Trips in the transit of Passenger Trains on the Accrington Incline.*

| SINGLE ENGINE. | | | | | DOUBLE ENGINE. | | | |
|---|---|---|---|---|---|---|---|---|
| No. of Trips. | Mean weight carried in Tons. | Rate of travelling in miles per hour. | Power of Engine represented by the area of cylinders. | | No. of Trips. | Mean weight carried in Tons. | Rate of travelling in miles per hour. | Power of Engine represented by the area of cylinders. |
| 7 | 20 00 | 20·00 | 353·4 | | 16 | 26·00 | 21·35 | 755·4 |
| 12 | 20·83 | 23·00 | 353·4 | | 4 | 25·25 | 19·20 | 706·8 |
| 5 | 29·00 | 15·00 | 353·4 | | 6 | 25·16 | 20·30 | 706·8 |
| | | | | | 23 | 29·13 | 19·44 | 755·4 |
| | | | | | 5 | 31·60 | 16·60 | 706·8 |
| | | | | | 12 | 32·08 | 17·14 | 706·8 |
| | | | | | 18 | 28·30 | 18·40 | 755·4 |
| | | | | | 7 | 28·57 | 20·76 | 706·8 |
| | | | | | 8 | 29·60 | 16·85 | 706·8 |
| ... | 23.26 | 19.33 | 353.4 | Mean. | ... | 28·41 | 18·89 | 722·8 |

The Accrington incline is composed of three gradients and five curves, as follows:—

On this incline the curves are—

| | |
|---|---|
| 1. Bottom straight for ........................... | 21 chains. |
| 2. Curve to the right, 50 chains radius, for ... | 19½ „ |
| 3. Curve to the left, 50 chains radius, for ..... | 14 „ |
| 4. Curve to the left, 56 chains radius, for ..... | 41 „ |
| 5. Straight for ........................................ | 33 „ |
| 6. Curve to the right, 40 chains radius, for ... | 8 „ |
| 7. Curve to the right, 54 chains radius, for ... | 23½ „ |
| Total ............................... | 160 = 2 miles. |

On a careful examination of the returns as indicated in the above summary, very different results will be found to those contained in the preceding Tables, as derived from the goods trains. There the duty performed by the leading and assistant engines bears nearly a direct comparison with the single engine, both with respect to the weight carried and the speed attained. In passenger trains, where the load is light, and more within the power of the engine, the superiority of the single engine is strikingly apparent, and will not bear a comparison. It appears, when the load does not exceed 20 tons, the assistant engine is of little value; and it is only in monster trains, or in cases where the load exceeds the maximum power of the engine, that the assistant engine proves advantageous. In the experiments herein recorded, we have for the mean of three trips a load of 23¼ tons, drawn up an incline of nearly 1 in 40, by a single engine, whose powers are represented by 353, at the rate of 19·3 miles an hour; whereas, on a mean of 9 trips, with two engines, where the maximum power is represented by 722·8, a load of 28·4 tons (little more than the other) is carried the same distance, and to the same elevation, at the rate of only 18·89, rather under 19 miles an hour.

The result of these experiments is obvious. First; that in heavy trains, where the load approximates or exceeds the

power of the engine, the assistant engine under these circumstances becomes absolutely necessary; on the other hand, where the load is duly proportioned to the power, and that power so nicely balanced as to have full command over the resistance, we may then with great benefit, and no inconsiderable economy, dispense with every description of auxiliary force. In railway traffic these facts are worth knowing, and are fully borne out by the results thus obtained in working one of the largest and most difficult inclines in the kingdom.

In the experiments thus recorded, it may be interesting to ascertain how far they correspond with the results arrived at on the Hunt's Bank and Halifax inclines, and the computed resistances as given in the preceding tables. In cases of the latter it is estimated, that a locomotive engine of 16 inch cylinders would, at a speed of 33 miles an hour, raise a load of from 35 to 37 tons up an incline of 1 in 40. Now, in the case of the Accrington incline, if we compare the power of the single engine with 15 inch cylinders, which raised a load of 40 tons at the rate of upwards of 17 miles an hour up gradients, some of them steeper than 1 in 40, and allow for the retarding influence of curves, we shall then have a duty nearly equivalent to that in the table, if not approaching to the experiments made on the Hunt's Bank incline, where a much greater load was carried at a considerably reduced rate of speed. Viewing the subject in this light, and allowing for the increased pressure attained in experimental trials, compared to that of the regular working of the trains, we may reasonably conclude, that the differences are not so great as appearances at first sight would indicate.

In conclusion I would observe, that, notwithstanding the great improvements and increased powers which for the last fifteen years have been introduced into the locomotive engine, it would appear that we have not as yet arrived at that

maximum state of perfection which is calculated to ensure the conditions and meet all the requirements of surmounting steep gradients. These improvements are yet before us, and the object of the investigations which I have ventured to submit, will be fully attained if they lead to further enquiry into the conditions calculated to increase the powers and extend the resources of our locomotive traffic.

## XIII.—*On the Security and Limit of Strength of Tubular Girder Bridges constructed of Wrought Iron.* By William Fairbairn, Esq., V.P.

Read April 2, 1850.

Bridges have been in use from remote antiquity, and have received in all ages that consideration which the importance of the structure, and their great public utility, so justly entitle them to. They form the connecting link between one part of the earth's surface and another; allow of a continuous communication, by connecting the opposite banks of rivers and deep ravines, and overcome various obstacles which might otherwise be considered impassable. They, in fact, form a very important element in that system of communication by which the civilized nations of the world hold intercourse with each other, and which constitutes the medium of commercial interchange between the different districts of a country. They add facilities for the enjoyment of social life—for the easy direction of the necessary political supervision—and for that invaluable interchange of intellectual and physical relations, which contributes so largely to the wealth and intelligence of a nation. They, moreover, in modern times (associated with that wonderful development of iron "highways," which now traverse in every direction the surface of the country) constitute a medium of concentration in that union of distant objects, which is productive of so much benefit, and by which—through the aid of the locomotive engine—the remotest districts of the empire are now united.

These advantages are common to all countries; and now that rapidity of transit has become an essential part of our existence, it naturally follows that every discovery and every improvement which tends to the extension and enlargement of these facilities, must prove beneficial to the public, as well as interesting to the philosopher and the engineer. Impressed with these views, I have endeavoured to collect the results of a long series of experiments, and to narrow within the compass of a few pages those labours which have occupied no small share of my time and thought, whilst devising means for the construction and proportioning of the parts of a new system of bridge-building. I now propose to submit these results for the consideration of the Society, prefacing them by a few remarks relative to the construction and other matters connected with the security and permanency of this description of bridge.

In a paper given to the Institution of Civil Engineers I have stated, that "every erection of this kind, having for its objects public convenience and a public thoroughfare, should have within itself the elements of undeniable security. Bridges above all other structures should contain those elements: they are the most liable to accident; and, from whatever cause such accident may arise, the community are equally interested in the strength and durability of the structure. In attempting the introduction of a new system of construction, comprising the use of a new and untried material, it behoves the projector, therefore, on public grounds, to be careful and attentive to all the minutiæ directly or indirectly affecting its security. In bridges of the tubular construction, considerations of this kind are of primary importance, as much depends not only upon a correct application of the principle, but upon the quality of the material and the workmanship introduced, which, in every case, should be of the very best description. In the construction of Tubular Girder Bridges, I have endeavoured,

as correctly as possible, to apply those principles; and having a strong conviction of the great superiority of strength, durability, and cheapness which the system offers in compassing large spans, I have not hesitated to advocate its extension. It, however, becomes necessary, from time to time, to submit the bridges to a rigid examination; and, before opening any one of them as a public thoroughfare, it is essential to submit them to severe and satisfactory tests. These tests and examinations have been various and frequent; and I believe we may venture to affirm, that in no case where the Tubular Girder Bridge has been duly proportioned and well executed, has there been the least reason to doubt its security.

"The first idea of a Tubular Girder Bridge originated in the long preliminary experimental research which I conducted, in connection with the great bridges on the line of the Chester and Holyhead Railway; and, during its first application to railway constructions, the utmost precaution was observed in the due and perfect proportion of the parts. These proportions were deduced from the experiments made upon the model of the Britannia Tubular Bridge at Millwall, London; and after repeated tests upon a large scale (full size), the resisting powers, and other properties of the bridge, were fully established. From these experiments, a formula was deduced for calculating the ultimate strength of this tubular description of bridge, having spans of from 30 up to 300, or even 1000 feet; and, as that formula is now before the public, I believe it may be relied upon as practically correct. To relieve it, however, from any thing like ambiguity, I shall endeavour to state as briefly as possible, certain points which, in my opinion, should be taken into consideration in its application."

Experiments were made on a large scale to determine the accuracy of my views, and to ascertain the best and strongest form of tube as a means of supporting the Chester

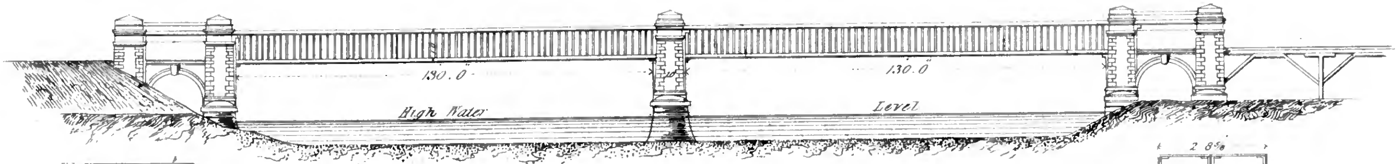
and Holyhead Railway, across the wide spans of the estuary of the Conway and the Menai Straits. The original conception of a huge wrought-iron tube, of a circular or elliptical sectional form, suspended in mid-air, and of dimensions calculated to allow of the passage of the locomotive and its accompanying train through its interior, yielded before the facts which these experiments brought to light, to the still more extraordinary and daring project of a colossal hollow beam, having within itself not only self-supporting powers, but a sufficient excess of strength to carry the weight of nearly a dozen railway trains. Beyond this, the experiments gave the rough outline to the system now under consideration, and which has already received, in an extended application, the sanction and approval of practically scientific men, and the confidence of the public.

The Millwall experiments not only successfully realized those objects, but they made us acquainted with other constructions of equal if not even greater importance, in the development of the tubular girder system, which is admirably adapted for almost every description of bridge; and, beyond comparison, infinitely more extended and more general in its application, than the form of tube which now spans the depths of the Menai Straits. It is this girder construction which I am anxious to bring before the meeting, in order to explain its peculiar adaptations, and to receive those suggestions for its improvement, which, I am satisfied, will be freely given by the members of this Society.

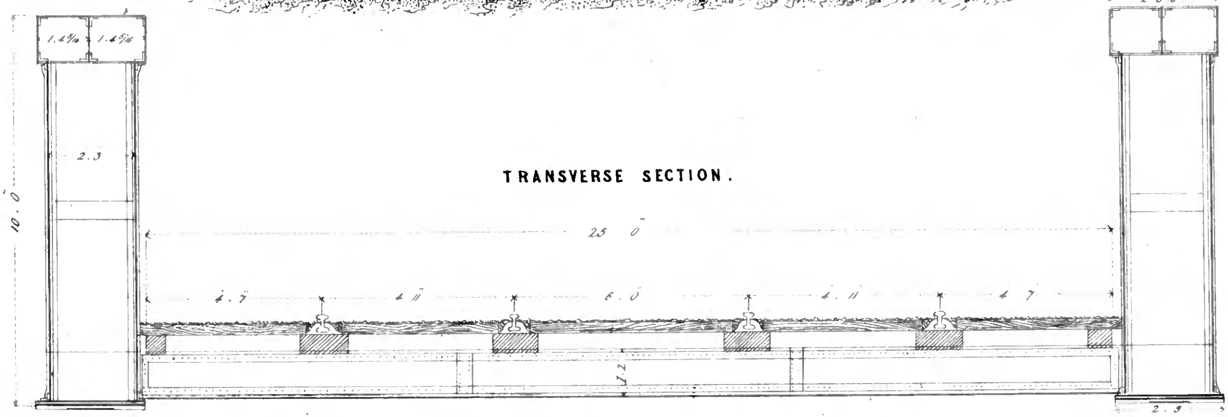
It was determined by the experiments, that, in order to balance the two resisting forces of tension and compression in a wrought-iron tubular girder having a cellular top (as shown in the plate), that the sectional area of the bottom should be to the sectional area of the top, as 11 : 12; and the proportional of these parts being thus established, it therefore follows, that any increase to one or other of them will not materially affect the strength of the bridge.

# TORKSEY TUBULAR GIRDER BRIDGE OVER THE RIVER TRENT.

GENERAL ELEVATION.




TRANSVERSE SECTION.




**Sectional Area in Top**

| | | |
|---|---|---|
| Plates | $2.8\frac{5}{8} \times 2 \times \frac{3}{8}$ = | 24 . 17 |
| Vertical do | $1.1\frac{1}{4} \times 3 \times \frac{5}{16}$ = | 12 . 42 |
| Angle Iron 4.75 | | 13 . 35 |
| Total Area in Top | | 50 . 24 |

**Sectional Area in Bottom**

| | | |
|---|---|---|
| 2 Plates | $2.9 \times 2 \times \frac{5}{8}$ = | 41 . 25 |
| Centre Strip | $1.0 \times \times \frac{3}{4}$ = | 9 . 0 |
| Packing Strip | $3\frac{3}{4} \times 2 \times \frac{5}{8}$ = | 4 . 68 |
| | | 54 . 93 |
| Deduct Rivets | | 5 . 25 |
| Total Area in Bottom | | 49 . 68 |

On the contrary, if additions be made to the one (assuming the ratio to be correct) without a proportional addition to the other, if the girder does not become absolutely weaker, it is evidently not increased in strength; inasmuch as increased dead-weight is given to the girder by the introduction of a quantity of material which is totally inoperative.* This being the case, it is of importance to preserve as nearly as possible the correct proportion of the parts, in order to ensure the maximum of strength in the two resisting forces of tension and compression, an arrangement essentially important in those structures; and also in the application of the formula to determine the utmost strength of the girder.† If, for example, an excess of material were given to the bottom of the girder shown in the plate, the formula ($W = \frac{a\ d\ c}{l}$) would not apply, inasmuch as the top and bottom areas would be disproportionate to each other, and the girder would fail from the yielding of the top before the stronger bottom

* It may be said that an increase of material to either top or bottom will increase its stiffness, and—*a fortiori*—its strength. I do not, however, admit this doctrine, as there is no telling to what extent these discrepancies may be carried, and the consequence of a disproportion of the parts, if once allowed, might lead to dangerous error. Besides, these proportions must either be correct or incorrect—if the former, any deviation from them is inadmissible.

† It is important to bear in mind, that in devising the formula for calculating the utmost strength of a tubular girder—which formula is, that the breaking weight is equal to the sectional area of the bottom multiplied by the depth, and by a constant derived from experiment for the particular form of girder under consideration, and the whole divided by the length—I have invariably assumed that the proportions which I have announced, and which were arrived at by frequent and direct experiment, are maintained; and further, that the constant which I have given is for a tubular girder constructed after these proportions, and with a cellular top. Other constructions would require other constants to be derived from experiment.

exerted its full resistance to the tensile strain. In estimating the dimensions for the application of the formula, the excess therefore would have to be reduced to the due proportion of 11 : 12 ; or, in other words, the additional strength must be left out of the calculation in computing the strength of the bridge. The same reasoning will apply when the excess of area happens to be in the cellular top, although in this case the formula ($W = \frac{a\ d\ c}{l}$) does apply, as the excess (in my opinion) goes for nothing in the calculation of the strength of the girder.

In every case, however, where these proportions are maintained, we have, in the above formula, a nearly correct principle on which to estimate the strength of similar wrought-iron tubular girders, whatever may be their relative dimensions.*

It must further be noticed, that in calculating the strength of bridges of this description, it is always assumed, that in addition to the proportions of the top and bottom of the girder being maintained, the vertical sides are sufficiently rigid to retain the girders in shape; and it is further assumed, that the whole of the plates, angle iron, &c., are in the line of the forces, and that the workmanship as well as the riveting is well executed.

* Mr. Tate, an eminent mathematician, remarks upon the formula—

1st. With respect to ($W = \frac{a\ d\ c}{l}$), where A is the area of the section of the bottom, and C = 80, the constant deduced on this supposition will apply to all depths of the tube within short limits of error where such depths, or A, are large in proportion to the depths of the cells and the thickness of the plates.

2nd. With respect to the formula ($W = \frac{a\ d\ c}{l}$), when A is the area of the whole section, and C = 26·7, then the tubes should be similar in all respects; but a slight variation in depth from that of similar form will not produce much error, especially where the depth is considerable. At the same time, it must be observed that both formulæ apply with great exactness where the tubes are similar.

At a recent discussion on this subject, which occupied two successive meetings of the Institution of Civil Engineers, Westminster, it was maintained that my formula was not correctly applicable in cases of girders of more than one span, and that I had neglected in the calculations the great increase of strength which was derived from the girders being continuous.

This continuity of the girder was estimated by some to add not less than one-third, and by others one-fourth, to the ultimate strength of that part of it which formed a single span, when viewed simply as a beam supported at the ends, as exhibited in the model now before you. On this question, I observe in a note appended to my paper read at the Institution of Civil Engineers, "that the doctrine of continuity is doubtless true to a certain extent; and, although I admit the fact, I have purposely neglected in the calculation any auxiliary support of that kind as a counterpoise," &c.—I think it safer to do so, as any admission of increased strength in that direction, might lead to serious practical inconvenience, if not dangerous results. I have therefore freely given, as additional security, those advantages of strength, whatever they may be, rather than adopt refinements in the calculation, which, if exercised by the general practitioner, might lead to serious error in reducing the ultimate strength of the bridge. To give to a tubular girder bridge, of more than one span, the full benefit of the extra strength derived from the counterpoise of the girders on the opposite side, the girders would require to be differently constructed; and, in place of the joinings of the plates being prepared to resist compression throughout the whole length of the girders, the cellular top would require to be constructed for about two-thirds of the span in the middle of each girder on the principle of compression—and for a distance of one-sixth on each side of the pier on the principle of tension. In fact, it would require a complex series of constructive operations,

in order to meet all the requirements of varied strain to which horizontal girders of this kind are exposed.

Viewing the question in this light, it appears preferable to adhere to a general formula, and to give to the artificer a simple rule of extensive application, such as he may safely use without entering upon theoretical investigation, which more properly belongs to the mathematician than the man of practical science.

In offering these remarks, I am far from underrating the manifold advantages which we derive from the theoretical disquisitions of the mathematician. Every investigation for the elucidation or correction of existing formulæ by the test of the exact sciences must be highly valuable; but having corroborated certain facts by repeated trials and experiments on a large scale, and having found the formula from which the calculations were made, apply with remarkable precision to almost every extent of span, I am strongly inclined to adhere to its truth, and to place implicit confidence in the construction obtained from such a source. I hope, however, that the time is not far distant when we may receive from some able mathematician a preferable and more accurate formula, if such can be obtained.

It may, however, prove instructive if we examine this question more closely, and endeavour to ascertain the real value of the additional strength thus imparted to each successive span by the continuous girder, and, for the sake of illustration, let us take the design of the bridge before us,* which has three spans, the middle being double the width of the two end ones, and consequently required to support double the weight. Now, it is evident that any considerable weight laid upon the centre of the large span of 250 feet, will cause a deflection; and, supposing the depth of the girder at the pier to be 14 feet, we then have 125 feet, or half the span, as the distance of the point of greatest de-

* The design for a Tubular Girder Bridge for supporting the Dublin and Belfast Junction Railway across the Boyne at Drogheda.

flection on one side of the pier, which, acting as the fulcrum, or support of the beam, has a tendency to raise, or tilt up the end of the land girder to the same height exactly from the abutment pier. Assuming this to be the fact, and the girder to be perfectly rigid, we should then have a tensile strain along the top side of the girder over the pier in the ratio of 125 : 14, nearly as 9 : 1. This is one of the advantages peculiar to the wrought-iron tubular girder, as, in every bridge having more than one span, the girders have always been made continuous; but as repeated changes are continually going forward from the passing trains, and as these changes, producing a severe strain, have a tendency to destroy the elasticity of the material, and the soundness of the workmanship at that part, I have considered it essential for the public safety to neglect it in the calculation, and to give in any additional strength which may arise from that source. Should it, however, be determined to take these advantages into account, a new formula must be deduced, and a new system of construction must be adopted over the piers, in order to attain the full benefit of this new element of strength.

The excess of strength that should be given to Girder Bridges, has received considerable attention not only from the profession, but also from the general public. The various accidents which have occurred in the failure of bridges of different constructions, have created of late years considerable alarm as to the stability of those important structures; and when the enormous weight of a railway train, and the momentum of that train moving at fifty miles an hour, are taken into consideration, it requires the utmost foresight, and the greatest possible care, to have the bridge sufficiently strong. These are considerations of deep importance to the engineer as well as the public; and although great difference of opinion exists as to the exact multiplier that should be given to the maximum load, to obtain the load which would produce rupture, I am of opinion that it should

never be less than four times the greatest load that can be brought upon the bridge. In the wrought-iron Tubular Girder Bridge, I have computed the breaking weight at twelve tons to the lineal foot, inclusive of the weight of the Bridge, which is equivalent to about six times the maximum load than can practically be brought upon it.

On this calculation, the following Table exhibits the strengths and proportions of Girder Bridges, from 30 up to 300 feet span. It has been computed from experiments on previously constructed Tubular Girder Bridges.

The first column gives the length of the span clear from pier to pier.

The second, the breaking weight of the bridge in the middle.

The third, the area of the plates and angle iron of the bottom of the girder.

The fourth, the area of the cellular top.

And the last, the depth of the girder in the middle.

TABLE

SHEWING THE PROPORTIONS OF TUBULAR GIRDER BRIDGES, FROM 30 TO 150 FEET SPAN.

| SPAN. | | Centre Breaking Weight of Bridge. | Sec. Area of bottom of one Girder. | Sec. Area of top of one Girder. | Depth at the Girder in the middle. | |
|---|---|---|---|---|---|---|
| Feet. | In. | Tons. | Inches. | Inches. | Feet. | In. |
| 30 | 0 | 180 | 14·63 | 17·06 | 2 | 4 |
| 35 | 0 | 210 | 17·06 | 19·91 | 2 | 8 |
| 40 | 0 | 240 | 19·50 | 22·75 | 3 | 1 |
| 45 | 0 | 270 | 21·94 | 25·59 | 3 | 6 |
| 50 | 0 | 300 | 24·38 | 28·44 | 3 | 10 |
| 55 | 0 | 330 | 26·81 | 31·28 | 4 | 3 |
| 60 | 0 | 360 | 29·25 | 34·13 | 4 | 7 |
| 65 | 0 | 390 | 31·69 | 36·97 | 5 | 0 |
| 70 | 0 | 420 | 34·13 | 39·81 | 5 | 5 |
| 75 | 0 | 450 | 36·56 | 42·67 | 5 | 9 |
| 80 | 0 | 480 | 39·00 | 45·50 | 6 | 2 |
| 85 | 0 | 510 | 41·44 | 48·34 | 6 | 7 |
| 90 | 0 | 540 | 43·88 | 51·19 | 6 | 11 |
| 95 | 0 | 570 | 46·31 | 54·03 | 7 | 4 |
| 100 | 0 | 600 | 48·75 | 56·88 | 7 | 8 |
| 110 | 0 | 660 | 53·63 | 62·56 | 8 | 6 |
| 120 | 0 | 720 | 58·50 | 68·25 | 9 | 3 |
| 130 | 0 | 780 | 63·38 | 73·94 | 10 | 0 |
| 140 | 0 | 840 | 68·25 | 79·63 | 10 | 9 |
| 150 | 0 | 900 | 73·13 | 85·31 | 11 | 6 |

## TABLE

### SHEWING THE PROPORTIONS OF TUBULAR GIRDER BRIDGES, FROM 160 TO 300 FEET SPAN.*

| SPAN. | | Centre Breaking Weight of Bridge. | Sec. Area of bottom of one Girder. | Sec. Area of top of one Girder. | Depth at the Girder in the middle. | |
|---|---|---|---|---|---|---|
| Feet. | In. | Tons. | Inches. | Inches. | Feet. | In. |
| 160 | 0 | 960 | 90·00 | 105·00 | 10 | 8 |
| 170 | 0 | 1020 | 95·63 | 111·56 | 11 | 4 |
| 180 | 0 | 1080 | 101·25 | 118·13 | 12 | 0 |
| 190 | 0 | 1140 | 106·88 | 124·69 | 12 | 8 |
| 200 | 0 | 1200 | 112·50 | 131·25 | 13 | 4 |
| 210 | 0 | 1260 | 118·13 | 137·81 | 14 | 0 |
| 220 | 0 | 1320 | 123·75 | 144·38 | 14 | 8 |
| 230 | 0 | 1380 | 129·38 | 150·94 | 15 | 4 |
| 240 | 0 | 1440 | 135·00 | 157·50 | 16 | 0 |
| 250 | 0 | 1500 | 140·63 | 164·06 | 16 | 8 |
| 260 | 0 | 1560 | 146·25 | 170·63 | 17 | 4 |
| 270 | 0 | 1620 | 151·88 | 177·19 | 18 | 0 |
| 280 | 0 | 1680 | 157·50 | 183·75 | 18 | 8 |
| 290 | 0 | 1740 | 163·13 | 190·31 | 19 | 4 |
| 300 | 0 | 1800 | 168·75 | 196·88 | 20 | 0 |

In the above Table it will be seen that I have adopted a large multiplier for the excess of strength which I conceive necessary to be observed in the construction of a railway bridge. Twelve tons per lineal foot, equally distributed over the surface of the bridge, is a heavy load as a measure of strength; and although I differ with some of my professional brethren in this question, I am nevertheless of opinion, that the difference of cost in effecting this object is inconsiderable when weighed against the additional security obtained.

In the wrought-iron tubular girder, the difference in the weight of the bridge itself is proportionally less than in any

* I have generally taken the depth of the girders at $\frac{1}{15}$ of the span; but in cases where the span does not exceed 150 feet, I have found it more economical to adopt $\frac{1}{13}$ of the span. With upwards of 150 feet span it is, however, more convenient, on account of the great height of the girder, to adhere to the original proportion of $\frac{1}{15}$, in order to keep the centre of gravity of the girder low, and in order to prevent oscillation to the passing load. In situations where it is objectionable to increase the depth of the girders, it then becomes essential to increase the sectional areas of the bottom and the cellular top in the ratio of the depths.

other construction; and, considering the risk of oxidation arising from neglect in attending periodically to the cleaning and painting of the girders, I am satisfied I am not wrong in making such a provision, and in substituting this large power of resistance for the strength of the principal parts of the structure. It is for these reasons that I have assumed for a double line of rails 12 tons per lineal foot as the ultimate strength of a Tubular Girder Bridge, calculated to ensure permanency, and to meet all the requirements of railway traffic. I have done so in order to meet the various contingent forces of the weight of the bridge itself, the maximum rolling load, and the various other conditions to which railway bridges are subjected, such as vibration or the force of impact acting injuriously upon the bridge.

Amongst other considerations which have engaged the attention of the commissioners on railway structures, is that of impact, and the effect of vibration upon bridges composed of cast-iron, either in the shape of the single or the compound trussed girder. The elaborate investigations on this subject, recently published, are exceedingly valuable; and, although they indicate several new and important properties in the strength of materials, they do not, so far as my own investigations extend, give the correct law as respects the effect of the impinging forces by which these structures are assailed. I believe Professor Willis (whose high standing as an acute mathematician is a sufficient guarantee for the accuracy of the experiments) is perfectly aware of this fact, and has qualified the experiments made at Portsmouth on cast-iron beams, nine feet long, by others upon existing bridges of not less than 50 feet span. These latter experiments are more satisfactory than those at Portsmouth, and approximate much nearer to those made by myself, and other experiments of a similar character.

The effects produced upon a girder bridge by a heavy body, such as a locomotive engine rolling over its surface

at a high velocity, is a subject of such vital importance to the permanency and stability of the structure, as to require the most careful investigation. It cannot therefore be surprising that it should have occupied a considerable portion of the time of the commissioners, and that it should have found a prominent position in their report.

It must, however, be observed, that the deflection of a girder bridge arises from one of two causes, or from both. First, from the weight of the bridge itself, which is a constant producing a permanent deflection ; and, secondly, from the passing load, whether viewed as a dead or a rolling weight, acting as an antagonistic force to the resisting power of the bridge.

In some parts of the commissioners' report, the experiments do not appear to me to bear out the facts of increased deflection produced by a body, such as a railway train moving at great velocity, and the same body remaining stationary, upon the bridge. In several carefully conducted experiments on tubular girder bridges of different spans, some of them upwards of 150 feet, I found the deflection as nearly as possible the same at all velocities; and, although the experiments recorded by the commissioners are highly valuable, they do not afford to the general practitioner those conclusive results which seem to be essential for the attainment of sound principles of construction. It is true, the commissioners in their report have qualified the results obtained from these experiments by others upon existing cast-iron railway girder bridges, where the deflection was reduced from an increase of the statical deflection, amounting to $\frac{9}{10}$ths of an inch, as produced upon the nine feet bars, at 30 miles an hour, to $\frac{1}{7}$ upon a bridge of 48 feet span, at 50 miles an hour, clearly showing that the larger the bridge, and the greater the rigidity and inertia of the girders, the greater will be the reduction of deflection to the passing load. In the tubular girder bridges composed of riveted

plates, it must be observed that the commissioners had no experience, nor were they acquainted with the strength, rigidity, and other properties of girders composed of wrought-iron riveted plates. In these, the deflection due to the passing load is nearly the same at all velocities; and unless there exist irregularities and inequalities on the rails, so as to cause a series of impacts, we may reasonably conclude that the deflections are not seriously, if at all, increased at high velocities.

The questionable security of a great number of horizontal bridges which, of late years, have been introduced for the support of railways, or common roads, has not only called for legislative interference, but the appointment of a commission to watch over the public interests and public safety in railway constructions. This commission, or the inspectors under their direction, I believe, have instructions to pass no bridge or other structure upon any line of railway, until carefully tested as to its security, and other conditions calculated to meet all the requirements of general traffic. These inspectors are employed for the exclusive purpose of examining every new line of railway, and reporting upon its efficiency before it is opened to the public; and, in order to assure themselves of the security of the bridges, cuttings, tunnels, embankments, &c., upon the line, these are generally submitted to severe tests, in order to ascertain their condition and fitness for securing to the public a safe and agreeable transit from one end to the other. Bridges, above all other structures, are regarded with suspicion, and, in order that the lives and limbs of the public should be duly protected, are submitted to a certain proof, which generally consists of a double train of locomotive engines and tenders being run over the bridge at different velocities. A train of locomotive engines is considered the greatest load that can be placed upon a bridge; and, having ascertained the deflection of the girders from their own

weight, and that of the roadway, the experiment generally proceeds as follows:—

First—To ascertain the increased deflection due to the heaviest load, as a dead weight placed upon the bridge.

Secondly—The amount of vibration produced by the passage of the same load at different velocities.

Thirdly—The amount of deflection due to the rolling load, and the variations, if any, when the trains are retarded or accelerated; and,

Lastly—The principle of construction is taken into consideration, and the excess of strength which a bridge should have over the greatest load, in order to declare it safe for general traffic.

On the first, second, and third, there appears to be little, if any, difference of opinion; but on the latter, the greatest and most opposite views are entertained. Some contending for three, and others for four, five, and six times the greatest load; whilst others again, more timid than the rest, insist upon eight or ten times the greatest load in order to be safe. Such appear to be the present views entertained by the profession, and such they will continue to be, unless decided by some high authority, from which there is no appeal, as to what should be the resisting powers of a bridge.

I make no doubt we have now in existence several bridges which, to all appearance, are duly performing the important functions of supporting heavily loaded trains within the narrow limits of probably half the weight that would lead to destruction; and others again are of such enormous strength as to bid defiance, for ages to come, to the heaviest load that can by possibility assail them.

Such I believe to be the present state of a considerable number of our railway structures, and such are the widely spread notions which have taken possession of some of our railway engineers. Under these discrepancies, it becomes a question of deep importance as to what should be the

exact measure of strength, and what excess should be given to a bridge beyond the load it is called upon to support.

It appears to be the opinion of the railway commissioners, that the flexure of girders should never exceed one-third of the ultimate deflection; and, although I concur in that opinion, I would venture to affirm that in wrought-iron tubular girders, such as are now in use, the effects of reiterated flexure is only one-sixth, and consequently they present a larger margin of security than girders composed of any other material.

On the effects of impact I entertain the same views as the commissioners, that the deflection produced by the striking body on wrought-iron is nearly as the velocity of impact, and those on cast-iron greater in proportion to the velocity. These facts have, however, been strikingly exemplified by experiments made on the first tubular girder bridges constructed for the support of a railway. Two bridges of this kind were erected near Blackburn, over the canal and turnpike road. Both bridges were 60 feet span, and before they were opened to the public they were subjected to the following tests:—

A train of three locomotive engines, weighing 60 tons, occupied the entire span of the bridge, and, having ascertained the deflection in their quiescent state, they were started at different rates of velocity, varying from 5 to 20 miles an hour, which produced a deflection of $\frac{3}{10}$ths of an inch. Two long wedges of the height of one inch were then placed upon the rails in the middle of the span, and the fall of the engines from this, when moving at a speed of 8 to 10 miles an hour, caused a deflection of only ·42 inch, which was increased to ·54 inch, or about half an inch, when wedges $1\frac{1}{2}$ inch in thickness were substituted.

These were severe tests, such as should not again be recommended, as the enormous strength of these girders is now well understood, and they may safely be considered fit for

service after being submitted to the heaviest rolling load, or one-sixth of the breaking weight.

In closing these remarks, I would observe that, after these experiments, I came to the conclusion that the tests for bridges of this kind should not exceed the greatest rolling load, and that load to be one-sixth of the breaking weight of the bridge. I may be wrong in this conclusion, which, with great deference, I submit for correction; but in this I am fully persuaded, that in order to give the necessary security, and to provide for all the contingencies consequent upon railway traffic, it will not injure the interests of a railway proprietary to have the bridges of sufficient strength to resist six times the greatest load.

---

### TORKSEY BRIDGE OVER THE RIVER TRENT.

The two main girders extend over the middle pier, on which they rest, with expansion rollers on each abutment.

| | Feet. | Inches. |
|---|---|---|
| Total length of each girder | 282 | 0 |
| Clear span of each opening | 130 | 0 |
| Depth of main girder | 10 | 0 |
| Breadth | 2 | 9 |
| Depth of cross girders | 1 | 2 |
| Span of ,, | 25 | 0 |
| Distance of ,, from centre to centre | 2 | 0 |
| Total length of bridge, including masonry | 342 | 0 |

| | Tons. | cwt. |
|---|---|---|
| Total weight of iron in main and cross girders | 252 | 14 |

#### PERMANENT LOAD FOR ONE OPENING.

| | Tons. | cwt. |
|---|---|---|
| Weight of main girders | 91 | 12 |
| Weight of cross girders | 27 | 1 |
| Timber | 18 | 5 |
| Ballast (2 inches thick) | 19 | 10 |
| Rails, chairs, and fastenings | 7 | 18 |
| | 164 | 6 |

| | Inches. |
|---|---|
| Sectional area of top, in inches | 50·24 |
| ,, ,, bottom ,, | 49·68 |

The bridge has been tested with six locomotive engines in steam, equally distributed over one opening, of the aggregate weight of 222 tons, when the deflection was found to be 1·26 inch in the middle. On the removal of the load the bridge returned to its original level.

## XIV.—*On the Cause of Unequal Falls of Rain in Cumberland.* *By* Alderman Thomas Hopkins.

Read October 1, 1850.

Mr. Millar, of Whitehaven, in a paper read to the Royal Society, on May 18, 1848, gives a number of important meteorological facts relating to the Lake district of Cumberland and Westmoreland. The statement of the falls of rain that take place in many parts of this locality are very valuable, on account of the different heights of the parts above the level of the sea where the rain gauges were placed, and the particular shape of the face of the country. For a long time it had been known that the fall of rain became greater, as the ground rose from the low level of Lancashire to the top of the ridge that separates that county from Yorkshire; and it appears that the same general fact is, to a certain extent, observable in Cumberland, Mr. Millar having found that the fall was small at Whitehaven and other places in the low country near the sea, compared with that which took place up the valleys and on the mountains of the interior country. And that gentleman, after stating many facts, attempts to exhibit a law which determines that the amount of rain shall increase up to a certain height, and decrease above that height. He says, "It seems probable that in mountainous districts the amount of rain increases from the valley upwards to an altitude of about 2,000 feet, where it reaches a maximum, and that above this elevation it rapidly decreases."—P. 85. Now, although the facts thus given are important in themselves, and afford a certain degree of countenance to the hypothesis advanced; yet neither

the facts nor the reasonings founded on them are sufficient to warrant the general conclusion drawn from them by Mr. Millar.

A return is given of the quantities of rain that fell in twenty different places in Cumberland in the four years 1845-6-7 and 1848; and of these places we may, in the first instance, take three as a sufficient number to shew how far the facts harmonize with the law laid down, namely, Whitehaven, Wastdale-head, and Seathwaite—the first being on the sea-coast, the second, inland, at the mouth of the mountain pass of Sty-head, and the third, beyond that pass, and in the valley of Borrodale. In these three places there fell in the years named the following quantities of rain, namely:—

| | At Whitehaven, 90 Feet above the Sea. | At Wastdale Head, 166 Feet above the Sea. | At Seathwaite, 240 Feet above the Sea. |
|---|---|---|---|
| | Inches. | Inches. | |
| In 1845 | 49·207 | 108·55 | 151·87 |
| „ 1846 | 49·134 | 106·93 | 143·51 |
| „ 1847 | 42·921 | 96·34 | 129·24 |
| „ 1848 | 47·344 | 115·32 | 160·80 |
| Mean | 46·589 | 106·60 | 145·63 |

Now, the differences in the heights of these three places are not very great, but the differences in the quantities of rain that fell are enormous—quite enough to warrant a suspicion, that the very large amount that fell at Seathwaite is not attributable to the height of that place above the sea. But in addition to those three places, there is the Pass of Sty-head, 1,290 feet high, situated between Wastdale-head and Seathwaite, on which a rain-gauge was placed; it is, however, so cold there in the winter, and the gauge is so much affected by snow and ice at that season, as to prevent reliance being placed on it during that portion of the year. Yet we may compare the quantities of rain that fell in the summer months only at Sty-head and Seathwaite, as given

by Mr. Millar—they are for the six summer months of 1848—

| | Inches. |
|---|---|
| Seathwaite, 240 feet above the surface of the sea,... | 68·96 |
| Sty-head, 1,290 feet above the surface of the sea,... | 60·35 |

Here we find no increase in the quantity of rain that falls above 240 feet of height where the gauge is placed in Seathwaite. On the contrary, the quantity is greater there than at Sty-head, 1,050 above it. This fact furnishes rather strong presumptive evidence, that the quantity of rain that is received in a gauge, at any particular elevation, is not proportioned to the height at which the gauge is placed.

In comparing the quantities of rain that fall at various heights, including great elevations, it is obviously necessary to compare them during the summer months alone, as has been done when comparing Seathwaite and Sty-head Pass; and the facts that are principally relied upon by Mr. Millar, and from which he draws his general conclusions, are the quantities of rain that fell in twenty-one months in 1846 and 1847 in six places, namely:—

| | | | Inches. |
|---|---|---|---|
| The Valley (Wastdale) ... | 160 | feet above the sea, | 170·55 |
| Sty-head,..................... | 1,290 | ,, | 185·74 |
| Seatoller,..................... | 1,344 | ,, | 180·23 |
| Sparkling Tarn,............ | 1,900 | ,, | 207·91 |
| Great Gable, ............... | 2,925 | ,, | 136·98 |
| Sca-fell, ..................... | 3,166 | ,, | 128·15 |

But these facts, although they countenance the hypothesis advanced, do not afford conclusive, or even strong, evidence upon the subject. The Valley we see, 160 feet high, has 170·55 inches; whilst Seatoller, 1,344 feet high, and consequently 1,184 feet higher than the Valley, has only 180·23 inches, not 10 inches more of rain; whilst Sty-head, 54 feet below Seatoller, has 5½ inches more of rain than that place. There is another place noticed by Mr. Millar, called Brant Rigg, 500 feet high, between the Valley and Sty-head, which received 12½ per cent. less of

rain than the Valley, that is only 160 feet high, showing that here less rain fell in the higher than in the lower parts; and there are other anomalies that might be pointed out. It is, however, such a place as Seathwaite that shews, in the most palpable and striking way, that the amount of rain that is received by the ground in a particular locality is not determined by its height. Seathwaite, not more than 240 feet above the sea, receives more rain than any of the places having a greater elevation; and Mr. Millar candidly admits that he is "unable to offer any satisfactory reason for the great excess of rain at Seathwaite over all other valleys;" and he might have said, over all other places in the locality, high as well as low.

In order to account for the great and unequal quantities of rain that fall in different parts of this district, it is necessary that we should briefly advert to the causes which determine the formation of rain at various heights in our atmosphere. The first, is the progressive diminution of temperature from the surface upwards, which is 1° for every 300 feet of height; and it follows from this, that any mass of the atmosphere saturated with aqueous vapour that is forced to ascend 300 feet will be cooled 1°—600 feet 2°—900 feet 3°, and so on progressively to greater heights, and the aqueous vapour that is intermingled with the air will be condensed in proportion to that cooling. If at any time condensation was slight on the low ground near the sea, it would become greater should the air be forced to ascend the valleys, and climb the sloping sides of mountains; and the greatest amount of condensation of vapour, and consequent formation of rain, would be at some certain height determined by the extent to which the air was saturated with vapour. The following figures will shew the heights at which vapour would be condensed under certain circumstances; that is to say, with the air and dew-point of the vapour at the surface both at 59°, when the tension of

vapour is equal to half an inch of mercury, the wind at the time blowing up a valley and sloping sides of a mountain. The lowest stratum of air being 59°, the temperature and dew-point would be reduced at a height of

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 300 feet | to | 58° | 1,200 feet | to | 55° | 2,100 feet | to | 52° |
| 600 | ,, | 57° | 1,500 | ,, | 54° | 2,400 | ,, | 51° |
| 900 | ,, | 56° | 1,800 | ,, | 53° | 2,700 | ,, | 50° |

And all the vapour that existed in the air between the dew-points of 59° and 50° would be successively condensed by the time that the air and vapour reached the height of 2,700 feet, and rain, the product of that amount of condensation, would be produced at the various heights as the cooling proceeded.

There is, however, a second process going on under such circumstances as those just described, which, as it modifies the first, it is necessary to notice. When condensation of vapour takes place heat is liberated, and the temperature of the locality is raised. The gases in the part are then warmed, and they expand and ascend to a greater height, where they are further cooled, and where they condense more vapour. So that the vapour is condensed in the first place by the atmospheric mass being forced up the inclined plane of the land, mechanically—as a wind—and, secondly, by the ascent produced by the heating power of condensing vapour; and whilst the mass of air and vapour is carried up from both these causes, it is moving forward horizontal as a wind. In the locality, then, the wind moves mechanically towards the upper part of the valley, whilst, from the heating effects of condensation, it is ascending above that part; the condensing vapour will therefore be liable to be carried above the highest part of the land, and the greatest quantity of rain may fall beyond that part. And, further—after the vapour has been condensed, and the rain formed at a certain considerable height in the atmosphere, it has to descend from that height, and will be liable, while so descending, to be

carried forward horizontally by the wind, and will reach the earth at a part beyond that over which it was formed.

Now, to apply this general statement and reasoning to the case under consideration, let us suppose that air saturated with vapour of the temperature of 59° passes from the sea-coast, near Ravenglass, towards the mountains as a south-west wind. When this wind reaches land 300 feet high, it will be cooled by ascent 1°, and will have all the vapour condensed that is contained between a dew-point of 59° and one of 58°. When it reached land 600 feet high, 2° of vapour would be condensed; 900 feet 3°, and so on in succession, 1° more for every 300 feet of height up which the air was forced mechanically. Add to this the vertical ascent produced by heat from condensation, and the actual progressive motion of the condensing vapour will be intermediate between the two. Supposing the two forces to be equal, the mass would proceed forward, ascending at the angle of 45°. Sty-head Pass is 1,290 feet high—the atmospheric mass, therefore, when it reached Sty-head would be cooled, say 4°, and it would be liable to be carried over the head of the Pass, rising at an angle of 45°. We might therefore expect, from the known laws of condensation of vapour, and of the action of wind, that, under the circumstances described, a larger amount of rain would fall beyond Sty-head, than either in the approach to it, or on the top of it; and accordingly it is the fact that a larger quantity of rain falls in Seathwaite, which is a little beyond the Pass, than in any part between Seathwaite and the sea.

In such a locality the saturated air, forced by the rise of the land to ascend, entering the wide mouth of the Valley, which contracts in breadth as it proceeds, rushes through the narrow gorge in the upper part, and over the top of the mountain pass, with great velocity and force; condensation therefore will take place to a greater extent along this particular and comparatively low line, than where the ridge

of the mountain is higher by 2,000 feet. The higher parts stop the passage of the wind, which makes its way where there is the least resistance, and this is over the pass of the mountain ridge; and as the horizontal rush of air is here particularly strong, any rain that is there formed, or that has been carried thither, will be liable to be borne forward until the air loses some of its velocity in the comparatively open space beyond the Pass where the rain is likely to be deposited—just as running water deposits sand when it reaches a wider and comparatively still part of a river.

In the case stated, the air was supposed to be saturated with vapour, but if it should not be fully saturated, but have a dew-point of, say 1° below the temperature, the only difference would be that condensation would not begin until the mass of air climbed 300 feet. When the dew-point is two degrees below the temperature, the air must ascend 600 feet before condensation begins; and the more the dew-point is below the temperature the higher must the air ascend before condensation will commence; but when it does begin, the process will be of the nature that has been described when the temperature and the dew-point were the same. And it should be recollected that it is not alone the vapour that is near the surface of the earth that may be condensed in the way described, but the whole vertical column may be affected in the same way as that part which is near to the surface. The vapour that is 300 or 600 feet distant from the surface of the low ground, may be equally raised and condensed with that which rests on the surface, seeing that the whole vertical column of the atmosphere may be raised by the obstruction presented by the mountain to the passage of the wind. And the rain that is formed in the higher, as well as that in the lower regions, is during its descent always liable to be carried forward by the wind.

We see, then, why the largest quantity of rain should fall in Seathwaite when a south-west wind blows from the sea

over Sty-head, as Seathwaite is favourably placed to receive much of the rain brought by that wind; but other winds blow in this district during a large portion of the year, and as much more rain falls at Seathwaite than in any other part, these other winds must, we presume, also bring rain to that place. To see how this is effected, we have to examine the shape of the neighbouring country, and particularly in the directions from which rainy winds come; and we may perhaps obtain a tolerably good idea of what that shape is from an account given in Hudson's *Guide to the Lakes.* In this work, page 118, it is said, "I know not how to give the reader a distinct image of the main outlines of the country, more readily than by requesting him to place himself with me in imagination upon some given point, let it be the top of either of the mountains, Great Gable or Scaw-fell; or rather, let us suppose our station to be a cloud hanging midway between those two mountains, at not more than half a mile's distance from the summit of each, and not many yards above their highest elevation, we shall then see stretched at our feet a number of valleys, not fewer than eight, diverging from the point on which we are supposed to stand, like spokes from the nave of a wheel." Now, this imaginary point in the air is nearly over Sty-head Pass. The writer then proceeds to describe Langdale, the Vale of Coniston, the Vale of Duddon, Eskdale, Wastdale, Ennerdale, and the Vale of Crummock water, and Buttermere. And he goes on to say, that "such is the general topographical view of the country of the lakes; and it may be observed that, from the circumference to the centre, that is, from the sea or plain country to the mountains specified, Great Gable and Scaw-fell, there is in the several ridges that inclose these vales, and divide them from each other—I mean, in the forms and surfaces—first, of the swelling ground, next, of the hills and rocks, and, lastly, of the mountains—an ascent of almost regular

gradation from elegance and richness to their highest point of grandeur and sublimity." Nearly all these eight valleys, in the low flat country, present wide openings to receive any wind that may be blowing towards them—they contract towards the centre where the ground rises; and the wind, whether it blow from, say, the south, south-west, the west, or the north-west, will force its way over the lowest points of the central chain, and be disposed to discharge rain on the country a little beyond those points. Borrodale is just in this situation, and must therefore receive rain from every moist wind that comes from a southern or western quarter, in the way that has been described; and Seathwaite seems to be in that part of Borrodale which receives the largest quantity of that rain.*

The large fall of rain in this village is then to be considered a result of various rainy winds blowing up the different valleys, and particularly those which lie to the south and west of it, as those winds force the mixed masses of air and vapour to rise to the lower parts of the elevated ridges that are at the heads of these valleys. At or above these parts the vapour is largely condensed, and the rain that is formed is carried forwards and deposited on the low ground beyond the ridge; but though deposited there it evidently descends from a great height.

That the height at which a rain-gauge is placed does not alone determine the amount of rain that falls into it, up to an elevation of 2,000 feet, is strikingly shown in the tables given by Mr. Millar for the three years ending with 1848. In 1846 October was the wettest month—in 1847 November, and in 1848 February was the wettest; and the mean quantities of rain received into the gauges at the following places in the wet months, to which I have added the mean of the years, are as follows:—

* Since this was written, another part that is contiguous has been found to receive more rain.

| | In Whitehaven, two places, namely, | | In Borrodale. |
|---|---|---|---|
| | High-street, 96 feet above the sea. | Round Close, 480 feet above the sea. | Seathwaite, 240 feet above the sea. |
| | Inches. | Inches. | Inches. |
| Months ... | 7·923 | 8·077 | 25·94 |
| Years ...... | 46·466 | 45·329 | 144·53 |

Here we see that whilst Round Close, which is 384 feet higher than High-street, receives only about the same quantity of rain that that place does, either in the wet months or in the year—Seathwaite, which is only 146 feet higher than High-street, receives on an average of both periods more than three times the quantity. And this particular fact is in harmony with the returns generally, showing that elevation alone towards 2,000 feet does not determine the amount of rain that shall fall into a rain-gauge.

Speaking in more general language, it may be said that the largest quantities of rain fall from warm and moist atmospheres, as such atmospheres contain the largest quantities of aqueous vapour; and the rain is formed by the condensation of a part of the vapour, at a height dependent on the elevation that is attained by the atmospheric mass when forced to ascend, and the difference between the temperature and the dew-point in that mass. When the dew-point is near the temperature at the surface, the largest quantity of rain will be formed at a moderate height. When the dew-point is more below the temperature, the largest quantity will be produced at a greater elevation; and when there is a great difference, or, in other words, when the air below is dry, should any rain be formed it will be at a great height, the particular locality in which the largest quantity of rain falls, being always more or less determined by the shape of the slopes of the land up which the air ascends. If the rise of the land is great and abrupt, approaching a vertical cliff, the larger part of the rain might possibly fall on the low ground in front of the cliff, the

mass of air being unable to pass over it, until such a height was attained as would leave little uncondensed vapour existing in the air. In such a situation it is evident, that one gauge placed at a low level in front of the cliff, might receive more rain than another fixed at any height above it. And it is equally clear, that when rain is formed whilst passing over an elevated ridge, that rain might be received either in a gauge placed beyond it, only a little lower, or in one not farther beyond it, but fixed in a deep valley below, as is, in fact, the case with the gauge at Seathwaite. We may therefore conclude, that in a country containing lofty mountains and deep valleys, with much irregularity of surface, the height of the gauge into which rain falls does not indicate the elevation at which it was formed—that elevation being determined by the laws of cooling of the aqueous vapour that is contained in our mixed atmosphere, whilst the vapour is diffused through the gases.

## XV.—*On Impossible and certain other Surd Equations.* *By* Robert Harley, Esq.

Read January 7, 1851.

1. The ordinary method of resolving a given surd equation proceeds on the assumption, that the symbol of radicality which enters into it may sustain indifferently either a positive or negative interpretation. It almost invariably happens, however, that from the nature of the enquiry whence such an equation originates, the sign of the radical is necessarily restricted to a *plus* signification; and that, therefore, every value of "the unknown," which, with this limitation, will not satisfy the given equation, is inadmissible as a root.

2. Now it is frequently found, that when the symbol $\surd$ is thus restricted in its signification, *all* the roots obtained by the ordinary method of solution, are rejective; that in fact they are *foreign roots*, belonging to one or more other equations which, when cleared of radicals, produce the very equation that results from the rationalization of the given one. These foreign roots are introduced by the elimination of the symbol of radicality from the proposed equation; for that elimination is effected either by multiplying by a factor or factors involving "the unknown," or by an evidently equivalent process.

3. That the removal of radicals does not eliminate from the given equation any of its roots, is easily proved. For let $\underset{1}{f}\ \underset{2}{f}$ be any rational functions whatever of $x$, connected by the equation

$$\sqrt{\underset{1}{f}} + \sqrt{\underset{2}{f}} = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1);$$

Then, multiplying by $\sqrt{f_1} - \sqrt{f_2}$, we get

$$f_1 - f_2 = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

a rational equation. Now, for whatever value of $x$ (1) obtains, (2) must likewise obtain; otherwise we should have

$$\sqrt{f_1} - \sqrt{f_2} = (f_1 - f_2) \div (\sqrt{f_1} + \sqrt{f_2}) = (f_1 - f_2) \div 0 = \infty;$$

$$\text{and } \sqrt{f_1} + \sqrt{f_2} = 0;$$

$$\therefore 2\sqrt{f_1} = \infty; \therefore f_1 = \infty; \therefore x = \infty,$$

which is absurd. Hence the proposition in relation to (1) is established.

Similar reasoning will evidently apply to any surd equation whatever. Thus, if we take the equation

$$\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3} = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3),$$

where $f_1 f_2 f_3$ are rational functions of $x$, we shall have

$$(\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3})(\sqrt{f_1} + \sqrt{f_2} - \sqrt{f_3})(\sqrt{f_1} - \sqrt{f_2} + \sqrt{f_3})$$
$$(-\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3}) = 0, \ldots\ldots\ldots\ldots\ldots (4),$$

$$\text{or, } f_1^2 + f_2^2 + f_3^2 - 2(f_1 f_2 + f_1 f_3 + f_2 f_3) = 0, \quad \ldots\ldots\ldots\ldots \quad (5),$$

a rational equation. Now, if there be a value of $x$ which will satisfy (3), and which will not also satisfy (5), or, which is the same thing (4), one at least of the factors $\sqrt{f_1} + \sqrt{f_2} - \sqrt{f_3}$, $\sqrt{f_1} - \sqrt{f_2} + \sqrt{f_3}$, $-\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3}$ must be infinite. Suppose

$$\sqrt{f_1} + \sqrt{f_2} - \sqrt{f_3} = \infty;$$

then, subtracting this equation from (3), we get

$$2\sqrt{f_3} = -\infty; \therefore f_3 = \infty; \therefore x = \infty;$$

which is absurd.

Cor. 1. Every value of $x$ which will satisfy (2), will also satisfy either (1) or its congener. For,

$$\because f_1 - f_2 = 0;$$

$$\therefore (\sqrt{f_1} + \sqrt{f_2})(\sqrt{f_1} - \sqrt{f_2}) = 0;$$

hence either $$\sqrt{f_1} + \sqrt{f_2} = 0,$$

or, $$\sqrt{f_1} - \sqrt{f_2} = 0, \text{ for all roots of (2).}$$

Cor. 2. In like manner it may be shown, that every value of $x$ which will satisfy the equation

$$f_1^2 + f_2^2 + f_3^2 - 2\,(f_1 f_2 + f_1 f_3 + f_2 f_3) = 0,$$

will also satisfy either one or other of the equations

$$\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3} = 0,$$
$$\sqrt{f_1} + \sqrt{f_2} - \sqrt{f_3} = 0,$$
$$\sqrt{f_1} - \sqrt{f_2} + \sqrt{f_3} = 0,$$
$$-\sqrt{f_1} + \sqrt{f_2} + \sqrt{f_3} = 0.$$

Cor. 3. If, when a surd equation is rationalized, and all the roots of the resulting equation are obtained, none of these roots are found to satisfy the proposed equation, that equation has no root whatever. For if such an equation had any root, that root would necessarily satisfy (and therefore be also a root of) the rational equation.

4. Definition.—An equation which has no root whatever, is designated as *impossible*. This definition is proposed by Mr. Cockle in the *Mechanics' Magazine*, Vol. xlix. (p. 365), where it is clearly demonstrated that the very supposition of the existence of such equations involves an arithmetical contradiction. It is needless, therefore, to argue the propriety of the definition, which we adopt without any hesitation.

5. For the purpose of clearly illustrating the preceding principles, let us consider the particular equation

$$4 + \sqrt{x - 3} + \sqrt{x + 21} = 0.$$

Multiplying by $(4 + \sqrt{x - 3} - \sqrt{x + 21})\,(1 + \sqrt{x - 3})$, in order to eliminate radicals, we get

$$8\,(x - 4) = 0;\ \therefore x = 4.$$

Now on trial we find that this value will not satisfy the proposed equation, unless the sign $(+)$ prefixed to the radicals be taken to mean the algebraic addition of *a* root of the quantities $(x - 3)$ and $(x + 21)$; that is (to be more explicit), unless we take the *plus* root of $(x - 3)$ and the *minus* root of $(x + 21)$. This hypothesis, however, I regard as inadmissible, believing it to be at variance with that definite and distinct signification of symbols, which is so absolutely essential to the *exact* expression of the conditions of a problem. It virtually transforms the sign prefixed to the radical into a mere connecting link, exercising no real control over that radical. It is true, that, when the symbol $\surd$ is introduced in the course of an algebraic investigation, it is always capable of sustaining both a *plus* and *minus* interpretation; so that in every such case the double sign $\pm$ is understood as involved in it. But this does not seem by any means to justify an *ambiguous*, or rather I should say a *variable*, interpretation of that symbol when it is necessarily employed with a particular sign of operation before it, that particular sign being indispensable to the perfect symbolic representation of the given conditions. In the one case, the symbol having been *introduced* for the purpose of effecting a transformation, may be taken both positively and negatively without violating any stipulated conditions: in the other case, it is *given* with a specific sign of operation prefixed to it, and this sign cannot be altered or evaded consistently with the conditions of the problem. For these reasons, therefore, we cannot accept 4 as a root of the proposed equation; but this is the only value of $x$ which will satisfy the rational equation: hence (*art.* 3, *cor.* 3)

$$4 + \sqrt{x - 3} + \sqrt{x + 21} = 0$$

has no root whatever, in other words (*art.* 4), it is an *impossible equation.* The *foreign root* 4 was introduced by

multiplying the equation by $4 + \sqrt{x-3} - \sqrt{x+21}$, for it satisfies the equation

$$4 + \sqrt{x-3} - \sqrt{x+21} = 0.$$

For the sake of further illustration, let the particular example

$$3x + \sqrt{30x - 71} = 5$$

be proposed. Eliminating the radical sign by the usual method, we get

$$9x^2 - 60x + 96 = 0,$$

which resolved, gives $x = 4$ or $\frac{8}{3}$. Now, neither of these values, when substituted for $x$, are found to satisfy the proposed equation; the equation really satisfied by them is,

$$3x - \sqrt{30x - 71} = 5.$$

We therefore conclude that

$$3x + \sqrt{30x - 71} = 5$$

is an impossible equation.

In Wood's Algebra by Lund (thirteenth edition), page 128, the equation we are now considering is discussed. After noticing the inadmissibility of the two values of $x$, above found as roots of the equation, the able Editor remarks, "whether there be any values of $x$ or not, which will satisfy the equation $3\,x + \sqrt{30x - 71} = 5$, we cannot say; all that we know is, that the common method of solution will not produce them." From what has been above shown, it is manifest that the doubt which is here expressed as to the *possibility* of the proposed equation, is altogether without foundation. It assumes, in fact, that the same value which satisfies the irrational equation does not necessarily satisfy also the rational one; but this assumption we know to be false. (Art. 3.)

6. In the algebraic solution of a certain class of problems, it is often of considerable importance to know *a priori*, whether the irrational equations which express the given conditions be possible or impossible, and (if possible) to

ascertain the exact number of roots belonging to each. A very little consideration will show, that for any surd equation of a given form to be impossible, a certain determinate relation must obtain among the co-efficients of $x$; and to discover that relation becomes at once an interesting and important enquiry. To find also a method of solution, equally applicable to all irrational equations, by which the true roots (if any exist) may be exclusively evolved, is plainly a very desirable object. These, then, are the two main purposes of the present paper; how far they are accomplished I shall not pretend to say.

7. I shall not now attempt to give a general discussion of this subject, but shall confine attention to certain surd equations of a limited degree. To illustrate the method I propose for the attainment of the objects specified in the last article, let us consider the literal equations

$$ax + \sqrt{bx + c} = d, \quad \dots\dots\dots\dots\dots\dots \quad (\alpha),$$

$$ax - \sqrt{bx + c} = d, \quad \dots\dots\dots\dots\dots\dots \quad (\beta).$$

These are readily put under the more simple and convenient forms

$$x_1 + \sqrt{2a_1 x_1 + b_1} = 0, \quad \dots\dots\dots\dots \quad (\alpha_1),$$

$$x_1 - \sqrt{2a_1 x_1 + b_1} = 0, \quad \dots\dots\dots\dots \quad (\beta_1),$$

where $x_1 = x - \frac{d}{a}$, $a_1 = \frac{b}{2a^2}$, and $b_1 = \frac{1}{a^3}(ac + bd)$.

Equation $(\beta_1)$ may be written thus:—

$$x_1 + \sqrt{2a_1 (-1)^2 x_1 + b_1 (-1)^2} = 0\,;$$

or, substituting $n$ for $-1$,

$$x_1 + \sqrt{2a_1 n^2 x_1 + b_1 n^2} = 0\,;$$

which agrees in form with $(\alpha_1)$. It hence appears that if $x_1 = f(a_1, b_1)$ be the solution of $(\alpha_1)$, the solution of $(\beta_1)$ will be $x_1 = f(a_1 n^2, b_1 n^2)$.

Let ($\underset{1}{\alpha}$) be multiplied by $\underset{1}{x} + n\sqrt{2\underset{1}{a}\,\underset{1}{x} + \underset{1}{b}}$; then, bearing in mind that $1 + n = 0$, we shall have

$$\underset{1}{x}^2 + 2\underset{1}{a}\,n\underset{1}{x} + \underset{1}{b}\,n = 0;$$

$$\therefore \underset{1}{x}^2 + 2\underset{1}{a}n\underset{1}{x} = \underset{1}{b}\,n^2;$$

$$\therefore \underset{1}{x} = \underset{1}{a}\,n^2 + \sqrt{(\underset{1}{a}\,n^2)^2 + \underset{1}{b}\,n^2};$$

$$\text{or, } \underset{1}{x} = \underset{1}{a}\,n^2 + n\sqrt{\underset{1}{a}^2\,n^2 + \underset{1}{b}}, \;\ldots\ldots\ldots\ldots\ldots \text{(1)}.$$

The second and third steps of the above solution may need, perhaps, a little explanation. In transposing the quantity $\underset{1}{b}\,n$ to the right-hand side of the equation, it will be observed, that instead of affecting it (according to the usual method) with the *minus* sign, we have *multiplied* it by $n$. That these operations are equivalent, is too evident to need demonstration; and it is easy to see also, that the introduction of the symbol $n$ for the negative sign is indispensable, to prevent the ambiguity that would otherwise result from the *obliteration* of that sign by involution. But it may be asked, would not the same ends be answered equally well, were we (instead of *multiplying*) to *divide* by $n$? In replying to this question, it is important to observe, that in order to enable us to retrace the several steps of the solution with unerring certainty, the symbol $n$ must always be employed in conformity with some invariable principle of operation; so that, by adopting an inverse principle, we may return with confident correctness, from any part of the investigation, through the successive steps, to the original equation. Unless the operation be thus conducted, it is obvious that ambiguity and error will attach to our results. In fact, we assume as the great principle that should guide us in the solution of surd equations, that every successive transformation should be made to bear with it an unmistakeable index of its immediate origin; for it is only by this means, we conceive, that those rejective roots (Art. 1) which enter into the ordinary solution may be excluded. Now, if we

recur to the first introduction of the symbol $n$ into the equation $(\underset{1}{\alpha})$, we find that it was employed as a *multiplier* of a negative quantity $(-\sqrt{2\underset{1}{a}\underset{1}{x}+\underset{1}{b}})$, in order that that quantity might be made to assume a positive form. Hence, therefore, in the foregoing transposition of $\underset{1}{b}\,n$, we must *multiply* (and not *divide*) it by $n$. So, in like manner, in extracting the root of the quadratic, half the co-efficient of $\underset{1}{x}$ is *multiplied* by $n$, and the square of the result is then added to the absolute term. Had we eliminated $\sqrt{}$ from $(\underset{1}{\alpha})$, by multiplying by $\underset{1}{x}+\frac{1}{n}\sqrt{2\underset{1}{a}\underset{1}{x}+\underset{1}{b}}$, instead of $\underset{1}{x}+n\sqrt{2\underset{1}{a}\underset{1}{x}+\underset{1}{b}}$, we should then have had to *divide* every transposed quantity by $n$, or (which is the same thing) to *multiply* it by $n^{-1}$: the value of $\underset{1}{x}$ so evolved would be found to differ from that above given only in form. In fact, since $n^p=-1$, $p$ being any *odd* number, positive or negative, we might (if we chose) employ $n^p$ for $n$, as a *multiplier*, throughout the investigation. Thus conducted, the operation would be as below:—

Multiplying $(\underset{1}{\alpha})$ by $\underset{1}{x}+n^p\sqrt{2\underset{1}{a}\underset{1}{x}+\underset{1}{b}}$, and bearing in mind that $1+n^p=0$, we have

$$\underset{1}{x}^2+n^p(2\underset{1}{a}\underset{1}{x}+\underset{1}{b})=0;$$

$$\therefore \underset{1}{x}^2+2\underset{1}{a}\,n^p\,\underset{1}{x}=\underset{1}{b}\,n^{2p};$$

$$\therefore \underset{1}{x}=\underset{1}{a}\,n^{2p}+\sqrt{\underset{1}{a}^2\,n^{4p}+\underset{1}{b}\,n^{2p}};$$

$$\text{or, } \underset{1}{x}=\underset{1}{a}\,n^{2p}+n^p\sqrt{\underset{1}{a}^2\,n^{2p}+\underset{1}{b}} \;\ldots\; (1^1).$$

It is easily seen that, since $p$ is *odd*, (1) and $(1^1)$ are virtually identical.

We now proceed to verify these solutions—

First, by (1,) $\sqrt{2\underset{1}{a}\underset{1}{x}+\underset{1}{b}}=\sqrt{\{2\underset{1}{a}^2n^2+2\underset{1}{a}n\sqrt{\underset{1}{a}^2n^2+\underset{1}{b}}+\underset{1}{b}\}}$

$$=\sqrt{\{(\underset{1}{a}^2n^2+\underset{1}{b})+2\underset{1}{a}n\sqrt{\underset{1}{a}^2n^2+\underset{1}{b}}+\underset{1}{a}^2n^2\}}$$

$$=\sqrt{\underset{1}{a}^2n^2+\underset{1}{b}}+\underset{1}{a}\,n \;\ldots\ldots\; (2)$$

(1) + (2) gives, $x_1+\sqrt{2a_1x_1+b_1}=(1+n)(a_1n+\sqrt{a_1^2n^2+b_1})$;

or, since $1+n=0$, $x_1+\sqrt{2a_1x_1+b_1}=0$, which verifies (1).

Secondly, by ($1^1$,) $\sqrt{2a_1x_1+b_1}=\sqrt{\{2a_1^2n^{2p}+2a_1n^p\sqrt{a_1^2n^{2p}+b_1}+b_1\}}$

$$=\sqrt{\{(a_1^2n^{2p}+b_1)+2a_1n^p\sqrt{a_1^2n^{2p}+b_1}+a_1^2n^{2p}\}}$$

$$=\sqrt{a_1^2n^{2p}+b_1}+a_1n^p \;\ldots\ldots\; (2^1)$$

($1^1$) + ($2^1$) gives, $x_1+\sqrt{2a_1x_1+b_1}=(1+n^p)(a_1n^p+\sqrt{a_1^2n^{2p}+b_1})$;

or since $1+n^p=0$, $x_1+\sqrt{2a_1x_1+b_1}=0$, which verifies ($1^1$).

The preceding solutions may be exhibited thus:—

$$x_1=n^2\;(a_1+\sqrt{a_1^2+b_1n^{-2}})\;\ldots\;(3),$$

$$\text{and } x_1=n^{2p}\;(a_1+\sqrt{a_1^2+b_1n^{-2p}})\;\ldots\;(3^1),$$

(3) and ($3^1$) corresponding respectively to (1) and ($1^1$).

From (3,) $\sqrt{2a_1x_1+b_1}=n\;(a_1+\sqrt{a_1^2+b_1n^{-2}})\;\ldots\;(4)$

" ($3^1$,) $\sqrt{2a_1x_1+b_1}=n^p\;(a_1+\sqrt{a_1^2+b_1n^{-2p}})\;\ldots\;(4^1)$

(3) + (4), and ($3^1$) + ($4^1$), each give

$$x_1+\sqrt{2a_1x_1+b_1}=0,$$

as it ought to be.

If now we write $-1$ for $n$, in (1) or ($1^1$), and bear in mind that $n^p=-1$, and $n^2=1$, we get

$$x_1=a_1-\sqrt{a_1^2+b_1}\;\ldots\ldots\ldots\ldots\ldots\ldots\;(5)$$

But if we make the same substitutions in (3) or ($3^1$), we get

$$x_1=a_1+\sqrt{a_1^2+b_1}\;\ldots\ldots\ldots\ldots\ldots\ldots\;(6).$$

(5) and (6) are in fact the values of $x_1$, which we should have found if we had solved $(\alpha_1)$ or $(\beta_1)$ by the ordinary method. Now we know from principles established in a previous portion of this paper (Art. 3), that no values of $x_1$, other than (5) and (6) can satisfy $(\alpha_1)$ or $(\beta_1.)$ And, since

(3) or ($3^1$) is identical (except in form) with (1) or ($1^1$), it immediately follows that (1) or ($1^1$) embraces all possible solutions of $(\alpha_1)$; and that, therefore, no loss of generality has been sustained by the exclusion of the negative sign from before the *introduced* radical. Indeed, this is as might have been expected from the logical and consistent character of the operation: it accords with that exact comprehensiveness of result, which must ever attach to those mathematical investigations which are conducted with due regard to the *entire* data of the problem discussed.

When the foregoing verifications involve the violation of our symbolical conventions, it is clear that the roots indicated by the formula (1) or ($1^1$) and (3) or ($3^1$) are rejective. But this can only be the case when the right-hand members of the equations (2) or ($2^1$) and (4) or ($4^1$) are negative (Art. 1). Now, still bearing in mind that the symbol $\sqrt{\phantom{x}}$ is to be interpreted positively, we readily discover, by mere inspection, that the right-hand member of (2) or ($2^1$) is negative only when $b_1$ is so; but that the right-hand member of (3) or ($3^1$) is always negative. We hence conclude that (6) can never strictly satisfy $(\alpha_1)$, and that (5) does so only when $b_1$ is positive. Combining these conclusions with what has been before demonstrated in this article, it is immediately seen that (6) is always a root of $(\beta_1,)$ and that (5) is so only when $b_1$ is negative.

These important conclusions may be deduced from other and more simple considerations. Thus, resolving $(\alpha_1)$ or $(\beta_1)$ by the common process, we obtain the relations marked (5) and (6.) Now from (5) we get

$$\sqrt{2a_1x_1 + b_1} = a_1 \backsim \sqrt{a_1^2 + b_1} \;\ldots\ldots\; (7);$$

And from (6) we get

$$\sqrt{2a_1x_1 + b_1} = a_1 + \sqrt{a_1^2 + b_1} \;\ldots\ldots\; (8)$$

It is scarcely necessary to say, that in finding these values of $\sqrt{2\,a_1\,x_1 + b_1}$, only positive results have been received.

Now, according as $b_1$ is positive or negative, we shall obviously have $\sqrt{a_1^2 + b_1} >$ or $< a_1$. Hence, (5) + (7) gives

$x_1 + \sqrt{2a_1x_1 + b_1} = 0$ or $2a_1$, according as $b_1$ is positive or negative.

In like manner, (5) — (7) gives

$x_1 - \sqrt{2a_1x_1 + b_1} = 2a_1$ or 0, according as $b_1$ is positive or negative.

Again, (6) + (8), and (6) — (8), give respectively,

$$x_1 + \sqrt{2a_1x_1 + b_1} = 2a_1;$$

$$\text{and } x_1 - \sqrt{2a_1x_1 + b_1} = 0.$$

From these results it appears, that (5) is the solution of either $(\alpha_1)$ or $(\beta_1,)$ according as $b_1$ is positive or negative; and that (6) is always a solution of $(\beta_1)$. Hence, also, by art. 3, cor. 3, and art. 4, when $b_1$ is negative, $(\alpha_1)$ is *impossible*, and $(\beta_1)$ has two roots, viz., (5) and (6).

We have already shown that (1), ($1^1$), (3), or ($3^1$) is a rigid symbolical solution of the equation $(\alpha_1)$; and yet we now find that neither (5) nor (6), which were both immediately obtained from that solution—by merely substituting for the symbol $n$ its arithmetical value—is necessarily a solution of that equation. To the experienced analyst, this seeming incongruity will be no matter of surprise. In Professor Young's "General Principles of Analysis," Part I., art. 8, a somewhat analogous case is elegantly discussed. The principle on which such seeming discrepancies as the one above alluded to may be satisfactorily explained, is there developed with that clearness of illustration and logical precision, for which that profound mathematician is so

2 F

deservedly celebrated. The general conclusion to which the Professor's reasoning tends, is thus elegantly expressed in the closing sentence of the article referred to:—"If, by any management or contrivance, we force, in a particular case, a violation of a *general* law, I need scarcely say that our result will be inadmissible." This remark is peculiarly applicable to the case now under consideration. For, by substituting for $n$ its numerical value in (1), the law of generation is lost sight of, and consequently, (5) and (6) being severally substituted in the irrational equation $(a_1)$, we violate, in a certain case already specified, the *general* law controlling the square root of expressions affected by the symbol $n^2$. To illustrate this clearly, let us consider the simple surd equation

$$1 + \sqrt{x} = 0 \text{ ........... } (a),$$

$$\text{Transposing, } \sqrt{x} = -1 = n:$$

$$\therefore x = n^2 \text{ ........... } (a^1).$$

Now $(a^1)$ evidently satisfies $(a)$; for $1 + \sqrt{n^2} = 1 + n = 0$; and yet, though $n^2 = 1$, $x = 1$ is not the root of $(a)$, but of its congener,

$$1 - \sqrt{x} = 0 \text{ .......... } (b).$$

The reason is, that $\sqrt{1}$ and $\sqrt{n^2}$ are not equal, the latter being $n$ times the former.* So, in like manner, by substituting from (3) and (6), we get respectively (4) and (8);

* Possibly it may be objected that $\sqrt{1}$ is *either* $+ 1$ *or* $- 1$, and that, therefore, unity is the root of both $(a)$ and $(b)$. In answer to this, it might seem sufficient simply to refer to Art. 5, in which it is shown, we think, that such a conclusion is not consistent with rigorous reasoning; that it involves, in fact, a virtual violation of the law of signs. As, however, the entire theory of impossible equations depends for its existence on the non-identity of such equations as $(a)$ and $(b)$, we may further remark, that if these be treated as simultaneous equations, $(a) + (b)$ will give $2 = 0$, an arithmetical absurdity. Whether or not $n^2$ is philosophically admissible as a root of $(a)$, will be hereafter considered.

but (6) was immediately derived from (3) by writing for $n$ its numerical value, and yet if in (4) for $n$ we substitute its value, and obliterate all even powers of unity, we get

$$\sqrt{2\,a_1\,x_1 + b_1} = -(a_1 + \sqrt{a_1^2 + b_1}),$$

which does not agree with (8.) The fact is, the expression following the negative sign in the right-hand member of this equation being necessarily positive, the equation itself is a clear violation of our symbolical conventions, and is therefore inadmissible. This sufficiently explains the reason why certain expressions in terms of $n$, *seem* to satisfy (and *algebraically do strictly satisfy*) certain irrational equations, which are nevertheless *impossible*. Thus, the equation ($a$) is easily shown to be impossible (see Art. 3, cor. 3), and yet it is strictly satisfied by $x = n^2$. Further discussion of this part of the subject I leave until I come to speak of *impossible expressions*, with which, as will be seen, it is closely and intimately connected.

It has been demonstrated that the root of ($\beta_1$) in terms of $n$, may be at once deduced from that of ($\alpha_1$) by writing $a_1\,n^2$, $b_1\,n^2$, for $a_1$ $b_1$ respectively. We thus get

$$x_1 = a_1\,n^4 + n^2\,\sqrt{a_1^2 n^4 + b_1}, \quad \ldots\ldots\ldots\ldots\ldots \quad (9),$$

$$\text{and } \therefore \sqrt{2a_1\,x_1 + b_1} = a_1\,n^2 + \sqrt{a_1^2 n^4 + b_1}, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (10),$$

$$\therefore x_1 - \sqrt{2a_1\,x_1 + b_1} = (n^2 - 1)\,(a_1 + \sqrt{a_1^2 n^4 + b_1}) = 0;$$

which verifies the solution (9). The invariable possibility of equation ($\beta_1$), and all the other conclusions which have been established with regard to that equation and its congener ($\alpha_1$), are immediately deducible from the above solution.

Of course (9), like equation (1), from which it is derived, may be exhibited in a variety of forms; these, however, it is needless to develop.

Adapting the foregoing results to the original equations, we have the following conclusions:—

The equation ($\alpha$) is possible or impossible according as $\frac{1}{a^3}$ $(ac + bd)$ is positive or negative, and ($\beta$) is always possible.

When $\frac{1}{a^3}$ $(ac + bd)$ is positive, ($\alpha$) has one root, viz.,

$$x = \frac{1}{2a^2}(b + 4ad - \sqrt{b^2 + 4abd + 4a^2c}), \ldots\ldots\ldots\ldots (\alpha');$$

and ($\beta$) has one root, viz.,

$$x = \frac{1}{2a^2}(b + 4ad + \sqrt{b^2 + 4abd + 4a^2c} \ldots\ldots\ldots\ldots (\beta');$$

when $\frac{1}{a^3}(ac + bd)$ is negative, ($\alpha'$) and ($\beta'$) are both roots of ($\beta$), and ($\alpha$) is impossible.

These are useful criteria, as they enable us by mere inspection to ascertain *a priori*, whether any proposed irrational equation of the form ($\alpha$) or ($\beta$), having numerical co-efficients, be possible or impossible; and likewise to determine, when the equation is possible, which of the roots, furnished by the usual process of solution, do, and which do not belong to the proposed equation.

Thus, let the equation

$$3x + \sqrt{30x - 71} = 5$$

be proposed. (See Art. 5.) Then, since this agrees in form with ($\alpha$), and in this case $\frac{1}{a^3}$ $(ac+bd)$ is negative, we at once pronounce it to be impossible. The roots 4 and $\frac{8}{3}$, given by the usual method of resolution, belonging to the corresponding equation

$$3x - \sqrt{30x - 71} = 5.$$

In practice, perhaps, it is more convenient to place the equation under the form ($\alpha_1$) or ($\beta_1$). Thus, the equation above proposed may be written as below:—

$$(3x - 5) + \sqrt{10\,(3x - 5) - 21} = 0,$$

and since the last term under the radical is *negative*, we conclude immediately that the equation is impossible.

Again, let the equation

$$x + \sqrt{x + 2} = 0$$

be proposed. This agrees in form with $(\underset{1}{\alpha})$, and since the second term under the radical is *positive*, the equation is *possible*, having one root, viz.,

$$\tfrac{1}{2} - \sqrt{\tfrac{9}{4}} = -1\,;$$

the root of its congener is

$$\tfrac{1}{2} + \sqrt{\tfrac{9}{4}} = 2.$$

It is worthy of remark here, that when $(\underset{1}{\alpha})$ or $(\underset{1}{\beta})$ is solved by the common process, the positive root belongs to $(\beta_1)$, and the negative one to $(\alpha_1)$; and that, when both roots are positive, $(\alpha_1)$ is *impossible* and $(\beta_1)$ has *two* roots. The reason of this is at once obvious, from the following simple considerations—

$$\text{Since, by } (\underset{1}{\alpha}),\ \underset{1}{x} = -\sqrt{2\underset{1}{a}\,\underset{1}{x} + \underset{1}{b}},$$

$$\text{and, by } (\underset{1}{\beta}),\ \underset{1}{x} = \sqrt{2\underset{1}{a}\,\underset{1}{x} + \underset{1}{b}},$$

$x_1$ in the first instance must be *negative*, and in the second *positive*, otherwise we should have to subject it to incompatible conditions.

8. The method of solution explained and illustrated in the last article, is evidently capable of application to any class of surd equations whatever, provided only that such equations, when rationalized by the common method, are capable of algebraical resolution.

Whether we employ $n$ or $n^p$ ($p$ being any *odd* integral number, positive or negative,) for -1, we have seen that the results are virtually identical, and that nothing is gained as to generality by the use of one symbol rather than the other; while the employment of $n$ has the advantage over that of $n^p$ in point of simplicity and convenience. In practice, therefore, it will be found better to employ $n$ exclusively.

From what has been already done, it will have appeared evident that the chief value of $n$, as an element of operation, is this—that it enables us to discover certain expressions for $x$, which, in every circumstance, seemingly satisfy the proposed equation. The consideration of these expressions will readily enable us further to determine the relation that must subsist among the several co-efficients of $x$, in the given equation, in order to that equation being possible. This latter object, however, may be more easily effected, as we have seen, by simpler means.

I have spoken of the roots of $x$, in terms of $n$, as satisfying the proposed irrational equation *only in appearance.* I proceed to explain my meaning.

In the last article it was shown, that when $b_1$ is negative, the equation

$$x_1 + \sqrt{2a_1 x_1 + b_1} = 0$$

is *impossible;* that is, it has *no root whatever.* And yet we have also shown, that the expression

$$a_1 n^2 + n \sqrt{a_1^2 n^2 + b_1},$$

being substituted for $x_1$, seems to satisfy the equation in every circumstance. How are these conclusions to be harmonized? If we recur to the verification of the solution (1), we shall be furnished with a satisfactory explanation of this difficulty. We there find that the substitution of the above root in the expression $\sqrt{2a_1 x_1 + b_1}$, gives

$$\sqrt{2a_1 x_1 + b_1} = a_1 n + \sqrt{a_1^2 n^2 + b_1}.$$

Now it has been before remarked, that when $b_1$ is negative, the right-hand member of this equation is negative also; but the left-hand member is always positive; hence, when $b_1$ is negative, the above expresses an *impossible relation;* viz., the equality of two quantities, one of which is positive, and the other negative. The root, therefore, which p sents

this incongruous result, is not strictly receivable. In like manner the root marked (3), viz.,

$$x_1 = n^2 (a_1 + \sqrt{a_1^2 + b_1 n^{-2}}),$$

though it appears to satisfy the proposed equation, is rejective, because it involves the acceptance of the *impossible equality*,

$$\sqrt{2a_1 x_1 + b_1} = n (a_1 + \sqrt{a_1^2 + b_1 n^{-2}}),$$

an equality which can have no more existence than the relation $1 = -1$. On the same grounds we reject $n^2$ as being strictly a root of

$$1 + \sqrt{x} = 0.$$

There can be no doubt, I think, that *algebraically* this value satisfies the equation, but *arithmetically* it does not; and to accept it, seems to me to be nothing less than an evasion of the authority of the sign $(+)$ prefixed to the radical.

An eminent analyst, to whose researches we have already had occasion to refer, in an interesting paper * published in the *Philosophical Magazine* for October 1850, seems to take a very similar view of this subject to that which we have just been expounding. After giving an elegant discussion of the equation

$$1 + \sqrt{x - 4} - \sqrt{x - 1} = 0,$$

Mr. Cockle remarks, "if, in the above instances, the difficulty is to be evaded, it is only by greatly refining our solution, and, as it has occurred to me, by using expressions of the form $m\,(+1)^2 + n\,(-1)^2$, and by following certain rules respecting our reductions, and the signs to be affixed to the radicals. To those who would attempt such a complex and artificial system of solution, rather than admit the

* "On impossible equations, on impossible quantities, and on tessarines. By James Cockle, Esq., M.A. of Trinity College, Cambridge; Barrister-at-law of the Middle Temple." Phil. Mag., third series, vol. 37, pp. 281-3.

existence of an impossible equation, I may hereafter address some observations. They will, however, probably find, as I have done, that their attempts are unsatisfactory, and their results not philosophically admissible. But I shall here content myself with remarking, that by any system of rules, however artificial, the difficulty is only thrown further back. Thus, the equation

$$\sqrt{x} + \sqrt{x+1} = 0$$

is utterly intractable."

After what I have already written, it is scarcely necessary to say, that in the opinion so elegantly expressed in the passage cited I entirely concur, and that I cannot but consider the existence of impossible equations an undoubted fact. With regard, however, to the equation proposed by Mr. Cockle, I may remark that, peculiar as it is, the method of solution explained in this paper, is applicable even to it. For, since

$$\sqrt{x} + \sqrt{x+1} = 0 \text{ ........ } (1)$$

$$\therefore \sqrt{x} = n\sqrt{x+1};$$

$$\therefore x = n^2 x + n^2;$$

$$\therefore x = \frac{n^2}{1-n^2} \text{ .........}(2);$$

substituting (2) in (1), we have

$$\frac{n}{\sqrt{1-n^2}} + \frac{1}{\sqrt{1-n^2}} = \frac{\sqrt{1+n}}{1-n} = \frac{\sqrt{o}}{2} = 0,$$

which verifies the solution (2). Still, it will be remarked, that the condition

$$\sqrt{x} = \frac{n}{\sqrt{1-n^2}},$$

which has been admitted into the verification, is incompatible with the restriction imposed on the symbol of radicality; and that, therefore, if the views which we have taken of the office of the signs (+ and —) prefixed to $\sqrt{}$ be correct, the *numerical value* of (2), viz., $\frac{1}{0}$, cannot be accepted as a

root of (1). This numerical value, it may be interesting to observe, is otherwise obtainable, thus:—Multiplying both members of (1) by $\sqrt{x} - \sqrt{x+1}$, we have

$$x - x - 1 = 0 ; \therefore 0x = 1 ; \therefore x = \tfrac{1}{0},$$

as before. This is evidently the root, however, of the equation

$$\sqrt{x} - \sqrt{x+1} = 0.$$

9. In a series of original essays, entitled, "Horæ Algebraicæ," published in the *Mechanics' Magazine*, Mr. Cockle has given a very interesting and general discussion of the theory of surd equations. *Horæ* VIII., IX., and X., contain valuable disquisitions on the algebra of impossibles; the history of which is given in the last-mentioned *Horæ*. To show that there is good ground for supposing that the existence of impossible equations was known, or at least *suspected*, by certain ancient philosophers, Mr. Cockle cites two very curious and interesting solutions from the *Vija-ganita*. My own remarks on those solutions I reserve until I have given Mr. Cockle's discussion, which is so interesting and instructive in all its parts, that I feel sure no apology will be deemed necessary for introducing it here *entire*.

"I am inclined to think,"* says Mr. Cockle, "but I offer the opinion with great hesitation, that the existence of impossible equations has been known for many ages—or, if *known* should seem too strong a word, I will state some circumstances which tend to indicate that the existence of such equations was at least *suspected* by those philosophers —whether Caucasian or Mongolian, Indo-German or Indian, or other, is not now the question—by those philosophers whose labours are preserved to us in the *Lilavati*,

* See *Mechanics' Magazine*, Volume xlix., pp. 555—7. Some of the foot-notes we omit as comparatively unimportant; and the rest, for obvious reasons, are transferred to the text, and bracketed.

the *Vija-ganita*, and the remaining records of ancient Oriental science. Of these circumstances, one is afforded by the first example of a quadratic equation, which occurs in the *Vija-ganita*. That example is as follows:—(see pp. 211–12 of Algebra, with Arithmetic and Mensuration, from the Sanscrit of Brahmegupta and Bhascara. Translated by Henry Thomas Colebrooke, Esq., F.R.S., &c. &c. &c. London: Murray, 1817. * * *)

"'The square root of half the number of a swarm of bees is gone to a shrub of jasmin, and so are eight-ninths of the whole swarm: a female is buzzing to one remaining male that is humming within a lotus, in which he is confined, having been allured to it by its fragrance at night. Say, lovely woman, the number of bees?'

"To solve this problem, we are directed to 'put the number of the swarm of bees *ya, v,* 2.' In this expression, *ya* is an abbreviation of *yavat-tavat*, of which the literal signification is, 'so much as,' and of which the meaning is *an unknown quantity*; *v* indicating that *ya* is to be squared, and 2, that twice the square is to be taken. In modern notation, this assumption would be represented by $2x^2$; but, the *quæsitum* of the problem being the number of bees in the swarm, why was not *ya* 1 (or $x$) taken to represent that number? It was not to avoid *fractions*, because, with that assumption, fractions occur in the statement of the question, and, moreover, such a purpose would only account for the occurrence of the co-efficient 2. As little was that assumption a capricious or accidental one, as we may see from the next example in the *Vija-ganita*. (Colebrooke, p. 212, Art. 133; and pp. 30—1,) which is:—

"'The son of Pritha, exasperated in combat, shot a quiver of arrows to slay Carna. With half his arrows he parried those of his antagonist; with four times the square root of the quiver full, he killed his horse; with six arrows he slew Salya; with three he demolished the umbrella,

standard, and bow; and with one he cut off the head of the foe. How many were the arrows that ARJUNA let fly?'

"The instructions which follow (p. 212) are:—'In this case, put the number of the whole of the arrows *ya, v,* 1.' In other words, assume that number to be $x^2$. But why not $x$? In this instance there appears to be but one answer for the Oriental investigator to give, viz., *we must avoid surds.* And this answer explains the peculiar form of the assumption in the first example. Had $x^2$ been assumed as the total number of bees, we should have had the surd $\sqrt{2}$ introduced into the expression of the problem. But, why object to the introduction of surds? It was not that the Orientals did not recognise surds; on the contrary, their knowledge of their properties was extensive and accurate (pp. 145-155). It was not that they had not a convenient notation, for such a surd number as $\sqrt{2}$ would be denoted by *ca,* 2, or (adopting Mr. Colebrooke's variation) by *c* 2; and, if we say with Narayana (page 145, note 1), that 'a quantity, the root of which is to be taken, is named *Carani,*' I cannot see why *ca* or *c* should not have been applied to *ya*—thus, *c, ya.* This quantity would have corresponded to our $\sqrt{x}$. The solution of the problem would then have been effected by our supposing the sum of *ya,* $\frac{1}{2}$, and *c, ya,* 4 and *ru,* 10 to be equal to *ya,* 1; and we should thence arrive at

[*c, ya,* 1 *ru,* 4]
[*c, ya,* 0 *ru,* 6],

whence we obtain 100 as the value of *ya;* and the further advantage that *yavat-tavat* is the very *quæsitum* of the problem, the number of arrows. But, even if we suppose the word *carani* to be exclusively applied to *number,* those who achieved in notation the results which we see in the *Vija-ganita* would not have had much difficulty in expressing the square root of *ya.* Is it improbable, then, that the

avoidance of surds, which, as we have seen, takes place in the above examples, became a rule of proceeding in consequence of the contradictory results to which surd equations sometimes lead us? *Bhascara* was aware of the double sign which attaches to a square root (p. 135), and has used that double sign to obtain two positive roots of a quadratic (p. 216), and I believe that he also admitted, in all cases, two roots of a quadratic; for we see him (p. 135), squaring a negative quantity, considered by itself, without reference to other quantities; and further, when we see him rejecting the root 5 because it is 'incongruous' (p. 217), he qualifies the rejection by, as I presume, assigning as a ground, that 'people do not approve negative absolute numbers,' and negative quantities are by means of this root 5, introduced into the conditions of the question. Now suppose for a moment, that, in the first attempts at the solution of the two problems given above, the *quæsitum* had been taken as *ya*, then the algebraist would have had in the first example, 72 and $\frac{9}{4}$, and in the second 100 and 4, as the values of *ya*. It would have been seen (for, as in other cases, both values would certainly have been tried), that the second value would in neither case satisfy the required conditions. I think it also highly probable, that the *reason* of the failure would have been seen, as the double value of the radical in the enunciation would naturally offer itself as a mode of explanation. The question then is, whether *ya* was originally taken as the *quæsitum* in those examples; and I confess that I cannot help seeing, in the introduction of square roots into the enunciation of the first two examples of quadratic equations, and in the assumption that *ya* is something other than the *quæsitum*, a marked desire to overcome a natural tendency to make such assumption—a tendency which the writer had found to lead to error. Add to this, that if the writer had considered the error in question as a mere 'incongruity,' he

would probably have noticed it as such, as he has done other 'incongruities' further on (pp. 217—18); but perhaps he felt that he could not call that an 'incongruity,' which was in fact a solution of the congeneric surd equation, corresponding to the given one. Lastly, let me observe, that we can hardly suppose that enquiry was directed to the two examples given above, and only to them. And the discovery of one surd equation without any root, would account for the studious avoidance of surd formulæ, which we see in the above portion of the *Vija-ganita*—a magnificent work, which would be more generally studied, did the history of science hold the position which it deserves in the estimation of the learned."

In the passage above cited, Mr. Cockle has, I think, shown good reason for suspecting that the ancient Oriental algebraists were not altogether ignorant of the existence of impossible equations; or at least of the fact, that in the solution of surd equations *foreign roots* are frequently evolved. The system of solution employed is admirably adapted for the exclusive determination of the possible root; and it is difficult to conceive why that system was so uniformly adopted, if it were not to avoid the contradictory results above referred to. A very slight extension of the ancient system will enable us to comprehend within it every irrational equation; for by making suitable assumptions, as we shall see, every such equation may be resolved into as many simultaneous rational ones as there are radicals and rational terms of $x$ in the proposed; and by rejecting all negative roots, the true and only value, or values, of the unknown will be determined by the common process. This system of solution, as we shall also see, has the subsidiary advantage of preventing the necessity for adopting the method of *experimental verification*, in order to ascertain to what equations the foreign roots introduced by the operation for eliminating radicals severally belong.

Let us recur to the examples cited by Mr. Cockle from the *Vija-ganita*. And first, as it regards the *bee question*, if, as directed, we denote the number required by $2x^2$, the conditions of the question will be expressed by the equation

$$x + \frac{16}{9} x^2 + 2 = 2x^2,$$

which gives $x = 6$,—the negative root being rejected; and consequently $2x^2 = 72$, the true and only answer.

But if $x$ (instead of $2x^2$) be taken as the *quæsitum* of the question, the equation will be

$$\sqrt{\tfrac{1}{2} x} + \tfrac{8}{9} x + 2 = x;$$
$$\text{or, } 2x - 9 \sqrt{2x} = 36 \;\ldots\ldots\; (A);$$

whence, by the usual process, we find $x = 72$ or $\frac{9}{2}$, the latter root being foreign, and belonging to the congener of (A), viz.,

$$2x + 9 \sqrt{2x} = 36 \;\ldots\ldots\; (B),$$

and the former root being alone the true answer.

Again; to solve the second example from the *Vija-ganita*, let us assume, as instructed, $x^2$ for the whole number of arrows; then we shall have

$$\tfrac{1}{2} x^2 + 4x + 10 = x^2;$$

whence $x = 10$, — the negative root being rejected as before; and $\therefore x^2 = 100$, the number required.

But taking $x$ (instead of $x^2$) for the number sought, we shall have

$$\tfrac{1}{2} x + 4 \sqrt{x} + 10 = x;$$
$$\therefore x - 8 \sqrt{x} = 10 \;\ldots\ldots\; (C);$$

whence, resolving as usual, we get $x = 100$, or 4. The former root satisfies (C), and is therefore the answer to the question; the latter belongs to the congener of (C), viz.,

$$x + 8 \sqrt{x} = 10 \;\ldots\ldots\; (D).$$

In passing, I may just observe that (A, B), (C, D), being put under the forms—

$$(2x - 36) \pm 9 \sqrt{(2x - 36) + 36} = 0,$$
$$(x - 10) \pm 8 \sqrt{(x - 10) + 10} = 0,$$

we immediately infer, from the fact of the last quantity under each of the radicals being positive, that the equations (A), (B), (C), (D), have each one, and only *one*, root. (See the latter portion of art. 7.)

Now, in each of the foregoing cases, the advantage of the *ancient* over the *modern* method, if we may so distinguish them, is too obvious to require argument. That method, as I have before intimated, is easily adapted so as to comprise within it every class of surd equations, and will always be found of important service when the sign of the radical is to be taken strictly as indicated. The consideration of a few particular examples, will sufficiently show the general applicability of the ancient system of solution to irrational equations, and will tend to illustrate more clearly than any number of general observations, the peculiar value of that system.

If the equation

$$4 + \sqrt{x - 3} + \sqrt{x + 21} = 0$$

(see art. 5), be proposed for solution, the ancient method at once suggests the following assumptions:—put

$$x_1 = \sqrt{x - 3}, \text{ and } x_2 = \sqrt{x + 21},$$

then we shall have

$$4 + x_1 + x_2 = 0,$$
$$24 + x_1^2 - x_2^2 = 0;$$

whence we readily find $x_1 = 1$, and $x_2 = -5$. Now the latter of these values, being negative, is rejective; and, since $x$ has only one root, we immediately infer that the equation is *impossible*. We likewise learn that the only *possible* equation analogous to the proposed one, is

$$4 + \sqrt{x - 3} - \sqrt{x + 21} = 0,$$

the root of which is $[x = x_1^2 + 3 = (x_2^2) - 21 =]$ 5. The

remaining corresponding equations, viz.,

$$4 - \sqrt{x-3} + \sqrt{x+21} = 0,$$

$$\text{and } 4 - \sqrt{x-3} - \sqrt{x+21} = 0,$$

being also impossible.

Again, let the equation

$$\sqrt{x^2+9} + \sqrt{25-x^2} - 2x = 0$$

be proposed. Assume $\sqrt{x^2+9} = x_1$, and $\sqrt{25-x^2} = x_2$; then

$$x_1 + x_2 - 2x = 0,$$

$$x_1^2 + x_2^2 - 34 = 0,$$

$$x^2 - x_1 + 9 = 0.$$

Eliminating $x$ between the first and third of these equations, we get

$$(x_1 + x_2)^2 - 4(x_1^2 - 9) = 0;$$

whence, by means of the second equation, we obtain

$$x_1 = 5, -5, \frac{7}{\sqrt{5}}, \text{ or } -\frac{7}{\sqrt{5}};$$

$$\text{and } x_2 = 3, -3, -\frac{11}{\sqrt{5}}, \text{ or } \frac{11}{\sqrt{5}}.$$

Now, since no corresponding pair of these roots, except the first, is positive, there is only one possible value of $x$ capable of satisfying the proposed equation, viz., $\left(\frac{x_1+x_2}{2}=\right)$ 4. The information afforded by the signs of the other roots is, that the equation

$$\sqrt{x^2+9} - \sqrt{25-x^2} + 2x = 0$$

has one root, and one only, viz., $\frac{2}{\sqrt{5}}$, and that the equation

$$\sqrt{x^2+9} + \sqrt{25-x^2} - 2x = 0$$

is *impossible.*

To recur to the equation

$$\sqrt{x} + \sqrt{x+1} = 0,$$

(Art. 8.) If we make $\sqrt{x} = x_1$, and $\sqrt{x+1} = x_2$, we shall have

$$x_1 + x_2 = 0,$$
$$x_1^2 - x_2^2 = -1;$$

whence we get $x_1 = -\frac{1}{2.0}$, and $x_2 = \frac{1}{2.0}$: the only value of $x_1$ being rejective, the equation is impossible; $x = x_1^2$ (or $x_2 - 1) = \frac{1}{4.0}$, is the solution of the equation

$$\sqrt{x+1} - \sqrt{x} = 0.$$

Examples of this kind might be multiplied indefinitely; these, however, are sufficient to show, that the method of the ancient algebraists (adapted) is admirably suited for the ready exclusion of foreign roots from the solution, and likewise for enabling us at once to determine, *without trial*, to what equations those rejective roots severally belong.

10. GARNIER seems to have been the first mathematician who distinctly affirmed the existence of impossible equations. In his *Analyse Algébrique*, p. 335, art. 92, he says, in speaking of the equation

$$-\sqrt{x-1} = 1 - \sqrt{x-4},$$

it "cannot be satisfied when the radicals are taken with the sign plus;"* and in the same place he further remarks, that "the operations by means of which the radicals are made to disappear, introduce roots foreign to the proposed" equation.*

Subsequently the subject received some attention from the late Mr. Horner of Bath, a gentleman whose valuable

* The above translations are taken from Mr. Cockle's *Horæ*, X., before referred to, and are followed by the subjoined remark:—"So that GARNIER must be understood as having distinctly asserted the existence of surd equations without roots, and also that the appearance of roots which such equations present, are the roots introduced by the processes through which we seek to rationalize the equations."—*Mechanics' Magazine*, vol. xlix. p. 557.

contributions to Algebra will ever occupy a conspicuous position in the history of that science. An interesting and instructive letter on the subject of surd equations, from Mr. Horner to Professor T. S. Davies, was published in vol. viii. s. iii. (pp. 43—50) of the *Philosophical Magazine.* In that letter, the mode in which foreign roots are introduced by the elimination of radicals is very clearly explained, and the existence of *rootless* or impossible equations satisfactorily demonstrated.

Some interesting remarks on the impossible equation

$$2x + \sqrt{x^2 - 7} = 5,$$

will be found on pp. 34, 35 of the *Gentleman's Diary* for 1837. This equation is also very elegantly discussed by Professor J. R. Young in the fourth edition of his valuable *Elementary Treatise on Algebra.**

Assuming that the symbol $\sqrt{}$ may be always interpreted, either positively or negatively, as circumstances may require, Mr. W. S. B. Woolhouse seems to contend that the doctrine of impossible equations is founded upon too restricted a view of that symbol. This question I have already sufficiently discussed, and only call attention now to Mr. Woolhouse's views, in order to make an opportunity of remarking, that much as I differ in opinion from that highly accomplished mathematician in the present instance, I consider his views on every mathematical subject as entitled to our best attention.

Of Mr. Cockle's researches I have already spoken. I believe he is the only mathematician who has taken any thing like a general or extended survey of the subject, and

* See pp. 131–2 of that admirable little work. It may be proper here to state, that Mr. Cockle, in the *Mechanics' Magazine*, vol. xlvii. p. 331, has taken objection to certain parts of Prof. Young's argument in relation to the equation noticed in the text, and that the Professor, with his characteristic frankness, has admitted the validity of Mr. Cockle's objections. —See *Mechanics' Magazine*, vol. xlvii. p. 546.

who has bestowed on it that degree of attention which its importance demands.

In concluding this paper, the author would adopt the language of the elegant writer last referred to in treating of the same subject:—"I hope that these investigations will not prove to be barren of results. At any rate, I trust that any efforts, however humble, to throw light on an anomalous—perhaps I may say mysterious—difficulty in algebra, will be regarded with toleration, if not with indulgence."

# XVI.—*On Impossible Equations.* By Professor Finlay.

Read February 4, 1851.

The following paper is intended as a supplement to Mr. Harley's paper on the same subject, read before this Society about a month ago, in which the fundamental principles of the theory were established, and some of the simplest cases of irrational equations were solved, in a very elegant and direct manner. My paper is divided into five paragraphs. The first contains the definition of the new sense of the term "impossible," with some illustrations relative to that definition. The second and third paragraphs contain the discussion of an irrational equation containing a single radical of any order. The third and fourth paragraphs contain the discussion of an irrational equation containing two or more radicals of any order. The object of the discussion, in all cases, is to ascertain, *a priori*, the number of impossible roots which the equation contains, and to determine the possible roots exclusively of the impossible ones. Although the paper is extremely short, I should hope that what it contains is sufficient to show the method of separating the possible from the impossible roots in *any* irrational equation.

## I.

An *impossible equation* is one the roots of which are all impossible. In this definition, the term root, as applied to an equation, is used in the ordinary sense, and the

whole question turns on the new sense in which it has been proposed to use the term *impossible.*

To illustrate this point, let the equation

$$x + \sqrt{(4x + 1)} = 5$$

be proposed. Clearing this equation of surds, and solving it by the ordinary process, the roots are found to be 2 and 12. Now, if 2 be substituted for $x$ in the proposed equation, we obtain

$$2 + \sqrt{9} = 5,$$

which is obviously true; but if 12 be substituted for $x$, we get

$$12 + \sqrt{49} = 5,$$

which is evidently false, provided that the radical be restricted to a positive signification, or to its arithmetical value. On these grounds, it has been proposed to call 2 *a possible root* of the proposed equation, while 12 has been designated as an *impossible root.* Thus we see, that *an impossible root of an irrational equation is one which does not satisfy the equation when the radical which it involves is restricted to a positive signification.*

In extending the theory to imaginary roots, a difficulty occurs as to the positive signification of an expression of the form $\sqrt{(a-b\sqrt{-1})}$. For if

$$(\alpha - \beta\sqrt{-1})^2 = a - b\sqrt{-1},$$

we shall also have

$$(-\alpha + \beta\sqrt{-1})^2 = a - b\sqrt{-1};$$

so that $\sqrt{(a-b\sqrt{-1})}$ may be equal either to $+\alpha-\beta\sqrt{-1}$, or to $-\alpha+\beta\sqrt{-1}$, and it is not *immediately* evident which of these is to be taken as its positive signification. Now, in extending any algebraical rule to a case not originally contemplated, the extension must invariably be so framed that the new rule may include the original one as a particular case. According to this principle we must evidently take $+\alpha-\beta\sqrt{-1}$ as the positive signification of

$\sqrt{(a - b\sqrt{-1})}$; for if we took $-\alpha + \beta\sqrt{-1}$, when $\beta = 0$, we should have $-\alpha$ for the arithmetical value of $\sqrt{a}$, which would be inconsistent with our original assumptions and restrictions.

## II.

Let us now consider the equation

$$x + m\sqrt{(2ax + b)} = c \quad \text{.....................} \quad (1),$$

where $m$, $a$, $b$, $c$, denote given numbers, which may be positive or negative, fractional or entire. If we assume

$$\sqrt{(2ax + b)} = y \quad \text{.............................} \quad (2),$$

and therefore

$$2ax + b = y^2, \text{ or } x = \frac{y^2 - b}{2a},$$

the proposed equation becomes

$$\frac{y^2 - b}{2a} + my = c, \text{ or}$$

$$y^2 + 2amy = 2ac + b \quad \text{....................} \quad (3).$$

Solving this equation by the ordinary rule for quadratics, we get

$$y = -am \pm \sqrt{(a^2m^2 + 2ac + b,)} \text{ or}$$

$$y = -am \pm \text{R} \quad \text{.............................} \quad (4);$$

where, for the sake of brevity, we use R to denote the arithmetical square root of the quantity $a^2m^2 + 2ac + b$.

Now, if the radical in equation (1) be restricted to its arithmetical value, it is evident from (2) that $y$ must be positive; and therefore all negative values of $y$ must be rejected as leading to values of $x$, which are *impossible*. Thus we see, that the roots of (1) will be both *possible* when the roots of (3) are both positive, and both impossible when the roots of (3) are both negative; but when one of the roots of (3) is positive and the other negative, one of the roots of (1) will be *possible* and the other *impossible*.

*First*, Let $m$ and $a$ have the same signs; then $-ma$ is negative, and the lower sign must be rejected in equation (4), as giving a value of $y$ essentially negative.

($\alpha$.) When $2ac + b$ is negative, we have $R < ma$; hence, in this case, the second value of $y$ is also negative, and equation (1) is impossible.

($\beta$.) When $2ac + b$ is positive, we have $R > ma$; hence the second value of $y$ is positive, and points to a possible root of equation (1), which may be found as follows:—Substituting $-ma + R$ for $y$ in equation (2), we get $\sqrt{(2ax + b)} = -ma + R$,

$$\therefore 2ax + b = m^2a^2 - 2ma\,R + m^2a^2 + 2ac + b,$$
$$\text{or } x = m^2a + c -+ m\,R.$$

*Secondly*, Let $m$ and $a$ have contrary signs; then $-ma$ is positive, and if the upper sign be taken in equation (4), the corresponding value of $y$ will be positive. In this case, therefore, one of the roots of equation (1) is always possible, and may be found as above.

($\alpha$.) When $2ac + b$ is positive, $R > ma$; hence the second root of equation (3) is negative, and must be rejected as leading to an impossible value of $x$.

($\beta$.) When $2ac + b$ is negative, $R < ma$; hence the values of $y$ given by the formula (4) are both positive, and equation (1) has two possible roots, which may be found as above, or by the ordinary rule for quadratics.

It may be observed here, that when the roots of (3) are imaginary, the quantity $2ac + b$ is essentially negative; from which we see that when $m$ and $a$ have contrary signs, and the roots of (3) are imaginary, the roots of (1) are both *possible* in the new sense of this term, although they are both *imaginary* in the ordinary sense.

By taking successively $m = 1$, and $m = -1$, in equation (1), we obtain the two equations discussed by the author; so that this discussion of equation (1), appears to embrace all the results at which he had arrived up to the time of reading his paper.

## III.

The preceding method may be generalized with the utmost facility. Let us consider, for instance, the equation

$$X + \sqrt[n]{X'} = 0 \dots\dots\dots\dots (1),$$

where $X$ and $X'$ denote any rational and entire algebraic functions of $x$. If we assume

$$\sqrt[n]{X'} = y \dots\dots\dots\dots (2),$$

$$\text{and } \therefore X' = y^n \dots\dots\dots\dots (2'),$$

equation (1) becomes

$$X + y = 0 \dots\dots\dots\dots (1').$$

Eliminating $x$ between (1′) and (2′), we obtain an equation of the form

$$\varphi(y) = 0 \dots\dots\dots\dots (3),$$

where $\varphi$ denotes a rational and entire algebraic function of the quantity $y$, to which it is applied. Now, if the radical in equation (1) be restricted to a positive signification, it is evident from (2) that $y$ must be positive; and therefore the negative roots of (3) must be rejected, as giving impossible values of $x$. Consequently, if the number of negative roots in equation (3) be found by means of the theorem of Sturm, the number of impossible roots of (1) may thence be readily ascertained. Thus, if $p$ and $q$ denote the degrees of the functions $X$ and $X'$ respectively, it is evident from (2′) that every negative root in equation (3) will give $q$ impossible roots for equation (1).

## IV.

The same method may be applied to equations containing any number of radicals. For the sake of clearness, let us first consider the particular equation

$$m\sqrt{(ax + b)} + n\sqrt{(cx + d)} = f \dots\dots\dots (1).$$

If we assume

$$\sqrt{(ax + b)} = y,\ \sqrt{(cx + d)} = z, \dots\dots\dots (2),$$

$$\text{and } \therefore ax + b = y^2,\ cx + d = z^2 \dots\dots\dots (2'),$$

equation (1) becomes

$$my + nz = f \text{ ......................... } (1');$$

and by eliminating $x$ from equations $(2')$ we get

$$bc - ad = cy^2 - az^2 \text{ ......................} (3).$$

Again, by eliminating $y$ from equations $(1')$ and $(3)$, we obtain

$$(cn^2 - am^2)\, z^2 - 2\, fncz = (bc - ad)\, m^2 - cf^2 \ldots (4.)$$

Let $z_1$ and $z_2$ be the values of $z$ deduced from this equation, $y_1$ and $y_2$ the corresponding values of $y$ deduced from $(1')$; then if each of the radicals in equation (1) be restricted to a positive signification, it is evident, that if either $y$ or $z$ be negative, the corresponding value of $x$ will be an impossible root of equation (1), and that the root of (1) corresponding to $y_2$ and $z_2$ cannot be possible unless $y_2$ and $z_2$ be both positive.

To illustrate this theory by a numerical example, let the proposed equation be

$$3\sqrt{(2x + 5)} + 4\sqrt{(3x - 2)} = 17.$$

Comparing this with equation (1), we have

$$m = 3,\ n = 4,\ a = 2,\ b = 5,\ c = 3,\ d = -2,\ f = 17;$$

hence equations $(1')$ and $(4)$ become

$$3y + 4z = 17,\ 30z^2 - 408z = -696.$$

From the latter equation we readily obtain

$$z_1 = 2,\ z_2 = \tfrac{58}{5};$$

and, by substituting these in the former, we get

$$y_1 = 3,\ y_2 = -\tfrac{147}{15}.$$

Now, since $y_2$ is negative, the corresponding value of $x$ will be an impossible root of the proposed equation; but since $y_1$ and $z_1$ are both positive, we see that the equation has a possible root. To find the possible root, let $z = z_1 = 2$ in the second of equations (2); then

$$\sqrt{(3x - 2)} = 2, \text{ and } \therefore\ x = 2,$$

which will be found to satisfy the proposed equation.

To find the impossible root, let $z = z_2 = \frac{58}{5}$ in the same equation; then

$$\sqrt{(3x - 2)} = \tfrac{58}{5}, \text{ and } \therefore\ x = \tfrac{3414}{75},$$

which will not satisfy the proposed equation, unless the first radical be taken with the positive, and the second with the negative sign.

## V.

The theory which has just been given for the case of two radicals of the second order, may now be readily extended to the case of any number of radicals of any order. For, if $t$, $u$, $v$, &c., be assumed equal to the several radicals, we shall obtain, in all cases, a series of equations free from radicals; and the number of these equations being always equal to the number of the unknown quantities $x$, $t$, $u$, $v$, &c., the values of $t$, $u$, $v$, &c., may always be found by the ordinary methods of elimination, at least when the proposed equation is numerical. Let $t_1$, $u_1$, $v_1$, &c., be any system of simultaneous values of $t$, $u$, $v$, &c., then it is evident that if $t_1$, $u_1$, $v_1$, &c., be all positive, the corresponding value, or values, of $x$ will be possible; but if any one of these quantities be negative, the corresponding values of $x$ will be impossible.

## XVII.—*On the Composition of the Gas produced by the Joint Distillation of Tar and Water at a high temperature.* *By* JOHN LEIGH, ESQ., M.R.C.S., F.C.S., &c.

Read January 21, 1851.

SOME time ago a process was brought under the notice of this Society by Mr. White, for the manufacture of illuminating gas from resin, tar, and other matters. The process had been made the subject of a patent, and was, I presume, brought before the Society on its scientific merits. It is not usual, I believe, for societies like this to receive and to discuss communications of patented processes for manufactures of any kind; but as it was supposed that an important scientific principle was involved in this method of Mr. White's, it was, I presume, on that account allowed to be received, and has on several occasions been very freely discussed in this room. As somewhat different opinions appear to have been entertained respecting it, I thought the communication of the results of a very carefully made analysis of the gas produced where tar was the material employed, might be interesting to the Society, and perhaps throw some light on the process itself.

It has long been known that when melted resin, or resin oil, is caused to fall upon, or trickle over, an extensive heated surface, that good gas is produced. A company was formed in London several years ago, for the manufac-

ture of resin gas. The melted resin was allowed to trickle over a quantity of coke heated to redness in a retort, and gas of an excellent quality was formed; but, after a large expenditure, the project was abandoned as too costly.

It is also well known, that when the vapour of water is passed over red-hot coke, it is decomposed, and converted into carbonic oxide and hydrogen. Professor Bunsen found in 100 volumes, 56 vols. hydrogen, 29 vols. carbonic oxide, and 14·8 vols. carbonic acid. It is evident from this analysis, that there are no products of the decomposition of water by red-hot coke or charcoal, that in themselves possess any illuminating power, and that there is no disposition on the part of the hydrogen, liberated under these circumstances from the water, to unite with the carbon with which it is in contact.

It has been stated by Mr. White, that he required three or four retorts, the gas from the water and from resin being formed in the extreme retorts, and brought into contact in the central ones. In these central retorts Mr. White assumed, that combination took place between the hydrogen of the water and the carbon or carburetted hydrogen of the resin, and on this it seemed to me rested the whole case; for, if no combination really took place, then was the gas produced simply resin gas, or tar gas, as the case might be. Whereas, if combination really did take place, there was a new element introduced into the process of gas-making, and possibly some value might have pertained to it.

It has long been known to chemists, that a union will often take place when gaseous bodies are brought into contact with each other in a nascent state, that is, at the moment that they are eliminated from some solid or liquid combination, and therefore, as it is presumed, before they have attained the full gaseous condition. This union is sometimes complete—and sometimes to a very limited degree complete—only when the affinity is relatively very strong, as in the

production of ammonia from the hydrogen and nitrogen of decomposing organic matters. Hydrogen and nitrogen, when fully formed, will never unite to form ammonia; when once the gaseous condition has been attained, there is no known means of making them combine. The same is the case with regard to carbon and hydrogen. Meeting in the nascent state, that is, being eliminated together, they combine and form various carburets of hydrogen, provided no stronger affinities, such as those of oxygen for hydrogen and carbon, are brought into play at the same time; for the stronger affinities will always be satisfied. But, notwithstanding the numerous experiments that have been made on the subject by able chemists, gaseous hydrogen has never been brought to unite with carbon. The analysis of the gases eliminated in the decomposition of water by red-hot coke, is a forcible illustration of this. In this case hydrogen is in contact with carbon under the most favourable conditions for combination, not only in the gaseous state, but in a nascent state, at the moment of its separation from the water, and yet no carburetted hydrogen is formed. Now, in Mr. White's process, the hydrogen is never brought in the nascent state in contact with the resin gas; but, after being fully formed in one retort, is brought in full gaseous condition into the retort for resin, and there expected to combine with carbon. But no such combination takes place; not a particle of hydrogen enters into union with the carbon.

The tendency of hydrogen to unite with carbon seems to diminish with elevation of temperature, whilst the reverse is the case as regards oxygen and carbon. Carbonic acid, $CO_2$, being passed over red-hot charcoal, takes up an atom of carbon, becoming 2 CO. But the higher carburets of hydrogen, under the same circumstances, deposit carbon. Benzole, $C_{12}H_6$, being conducted over red-hot quartz, deposits carbon, giving off olefiant gas, light carburetted hydrogen, and free hydrogen. Olefiant gas, again, deposits car-

bon, and yields light carburetted hydrogen and hydrogen; and, lastly, light carburetted hydrogen deposits its carbon, and escapes as free hydrogen.

I have had no opportunity hitherto of examining the gas produced by the joint distillation of rosin and water, but a full opportunity of examining that formed by the joint distillation of tar and water; Mr. White having requested permission from the Manchester Gas Committee to make some experiments in connection with an experimental apparatus which the Committee have fitted up at one of the Manchester gas stations. This apparatus is on a sufficiently large scale fully to test, in a practical manner, the value of any material required for gas-making; and, I may observe in passing, that before any cannel or coal is employed for gas-making by the Committee, it is tried in this apparatus. An entire oven of retorts is connected with a distinct set of receiving vessels for the tar and ammonia water, purifiers, washers, and gas-holder, and the gas is measured off by a separate meter. Numerous trials are made with the same cannel or coal, and an average of results obtained. The weight and quality of the coke is determined, the amount and specific gravity of the tar and ammonia water, and the volume of the gas produced. The latter is compared with a standard light, and its relative illuminating power and rate of combustion ascertained. Finally, it is submitted to a full and careful analysis by myself. The relative values of different gas-producing materials are determined, both in reference to the economy of their employment and the quality of the light they give.

With this apparatus, then, Mr. White's experiments were made, and every facility was afforded him by the servants of the establishment. When his experiments were concluded, I made an analysis of the gas produced, the results of which it is the object of this paper to lay before you.

PER CENTAGE COMPOSITION OF TAR AND WATER GAS.

| | |
|---|---|
| Olefiant Gas, or Condensible Hydro-carbon ... | 0·28 |
| Oxygen ... | 1·86 |
| Nitrogen ... | 13·83 |
| Hydrogen ... | 48·72 |
| Light Carburetted Hydrogen ... | 35·31 |
| | 100·00 |

All the volumes were reduced to 30 inches bar., and 60 deg. Faht. temp. The requisite corrections were also made.

I append analyses of gas made at the works in the ordinary way from Ince Hall Cannel, and Wood's Cannel (both from Wigan), respectively.

INCE HALL (Wigan) CANNEL GAS. Produce per ton, 12,440 cubic feet.

| | |
|---|---|
| Olefiant Gas and Condensible Hydro-carbon.... | 9·21 |
| Oxygen... | 0·16 |
| Nitrogen... | 5·37 |
| Hydrogen... | 42·33 |
| Light Carburetted Hydrogen ... | 38·08 |
| Carbonic Oxide... | 4·84 |
| | 99·99 |

WOOD'S CANNEL. Produce about the same.

| | |
|---|---|
| Olefiant Gas and Condensible Hydro-carbon.... | 9·00 |
| Oxygen,... | 0·00 |
| Nitrogen ... | 0·40 |
| Hydrogen... | 38·80 |
| Light Carburetted Hydrogen ... | 41·60 |
| Carbonic Oxide ... | 10·10 |
| | 99·90 |

The trials which were made at the station, where the tar and water gas was prepared, on the illuminating power, as compared with that from cannel, indicated a ratio of about 1 to 3. Consequently the tar and water gas ought to have yielded about three per cent. of olefiant gas to the analysis; whereas the proportion of this gas, on which the light-giving power mainly depends, is, in the gas now under consideration, only about a quarter of one per cent. It is

evident that the tar and water gas really contained no olefiant gas whatever; and that even the low illuminating power which it seemed to possess, when tried by the photometric test at the station where it was manufactured, was due to a little naphtha vapour which the gas held in mechanical suspension, and had not had time to deposit; whilst the sample I analysed, having been retained in a cool place for a time, had allowed of this deposition. Of course, if this gas had been sent into the town, the whole of the naphtha vapour would have been deposited in the pipes.

Another very remarkable fact is the entire absence of carbonic oxide in the gas, whilst this exists in the other two samples of gas, the analysis of which is quoted in the proportion of 4·84 and 10·10 respectively. If the water in these experiments had been in the slightest degree decomposed by the tarry matters, or even by the pitchy residue, carbonic oxide would have been one of the products.

What really took place in the process is now sufficiently simple. The naphtha from the tar, distilled over at once, and condensed in the gas-holder after a time, but a little remained dissolved or suspended in the gas for a brief period.

The less volatile portion of the tar decomposes, and is converted into light carburetted hydrogen and hydrogen. The large proportion of the latter would seem to show, that a small portion of the water employed in the process is decomposed by the iron of the retort rusting the latter, and yielding hydrogen.

The nitrogen in the gas proceeds from atmospheric air, and not from the materials employed.

Now the elements of tar are the same, but in different proportions, as those of resin, and exist in a state quite as fit to enter into combination with those of water; and these experiments prove infinitely better than an analysis of the resin gas itself even would do, that in the manufacture of

the latter no combination whatever takes place between the elements of resin and of water. This I have always maintained, and consider the proof now to be complete.

It may be said, that it matters little whether the combination here treated of, between the elements of water and of resinous or tarry matters, takes place or not; but it originally formed the whole question, for it was distinctly maintained, when the matter was first introduced into Manchester, that the great advantage of the process consisted in the fact, that such union did take place, and that therefore a quantity of olefiant gas was, as it were, created by this union, and hence it was called hydro-carbon gas, the name being intended to imply this union. I wish it distinctly to be understood, that I have considered this question simply as a scientific one—simply in its chemical bearings, without any reference to the cost of manufacturing resin gas, tar gas, &c.

---

## XVIII.—*On the Chemical Changes attending the Formation of Coal, and on the relation of these Changes to the Philosophy of Gas-making. By* JOHN LEIGH, Esq., M.R.C.S., F.C.S.

Read March 4, 1851.

As the rocks, above and beneath which it is imbedded, are the altered relics of the pre-existent mineral structures which, in countless ages past, formed the surface of this globe's crust; so is coal itself the altered relic of a vegetation which flourished in tropical luxuriance ere man and his congeners were called into existence. Some of the most glorious forms of vegetation that enrich the scenery of the south and east of this time's world, had then their analogues in this our northern clime: lofty trees reared their vast heights, lived out their time and fell, to be succeeded by others, till vast beds of vegetable matter accumulated, and with moss and fern formed wide morasses. Then peaty beds ensued, the remnants of a floral past, until the crust of earth gave way on which they rested; when ocean flowed where solid land had been, and fishes played and left their scales and bones upon the ocean's tangled vegetable bed; and rivers brought their sand and mud into the stiller deep, and covered all this up; and time rolled on, and rocks were formed above it, till earthquake's might projected them aloft, and laid all dry again; and thus, at last, the old world's plants now yield us light, and heat, and power immeasurable. Shut out from the atmosphere, but still, from their very depth, subjected to a temperature far above the average heat of this clime, the entombed beds of vegetable matter, screened from the more rapid agency of

oxidation, have for ages been undergoing a slow process of distillation, by which a portion of their more volatile constituents has been eliminated, the carburetted hydrogen and the carbonic acid escaping at every crevice, forming the fire-damp and choke-damp of the miner. The greater the extent to which this has taken place, the farther the coal is removed, in structure and properties, from the pristine vegetable matter from which it originated, till, at length, the carbon and ash alone remain, a mass of anthracite, which only burns like coke, and gives no gas. The oils which saturate the rocks in which it lies own the same source.

Thus we see that coal originates in vegetation; that, vitality departed, its elements are left to the play of their own affinities, exalted during ages of entombment by a temperature sufficient to carry off, by gentlest distillation and subsequent infiltration, the more volatile results of the new arrangements of those elements, undisturbed by the more active exercise of atmospheric agency; that coal, therefore, must differ from the original vegetable matter of which it was composed, by the loss of portions of its ingredients; that the remaining constituents, then, must exist in a different proportion and different relation to each other to what is found in fresh vegetable matter; and analysis proves this—analysis shows that coal is wood, minus a certain number of atoms of carbonic acid, water, and light carburetted hydrogen. And this gaseous elimination may proceed from the disunion and recombination of the elements of water in the vegetable matter.

It is in the elimination of oxygen, however, that coal chiefly differs from the original woody matter whence it is derived; for the relative proportion of carbon and hydrogen are not greatly different in coal and in wood. Woody fibre is a compound of 36 atoms of carbon, 22 atoms hydrogen, and 22 atoms oxygen. Cannel coal (some

varieties) is composed of 24 atoms carbon, 13 atoms hydrogen, and 1 atom oxygen, and 36 : 22 : : 24 : 14, so that the hydrogen exists in a slightly less relative proportion in cannel coal than in wood.

Every organized body, every organism, though not every body of organic origin—every plant and animal—essentially consists of four elements; hence often called organic elements—carbon, oxygen, hydrogen, and nitrogen. To these are superadded certain mineral substances also necessary to the existence of an organized body, forming, in fact, the skeleton of the plant or animal, and which, on the combustion of the body, are left as its ashes. The chief of these mineral ingredients are carbonate, sulphate, and phosphate of lime, to which may be added, in smaller proportion, compounds of potass, soda, magnesia, iron, and manganese.

When an organized body perishes, and decomposes with free access of air, as in the open atmosphere, its organic elements are converted into carbonic acid, water, and ammonia, the oxygen of the air assisting in the formation of the two former; these form the food and pabulum of a new vegetable existence, a constant cycle taking place of renovation and decay. It is manifest that, by the assistance of the atmospheric oxygen, the hydrogen and carbon are more rapidly and completely removed than when they are covered up. The composition of oak wood, which had been treated with water, alcohol, ether, diluted acids, and diluted alkalies in succession, to remove all extraneous matters but the woody fibre, was found, as stated above, to be—carbon 36 atoms, hydrogen 22 atoms, oxygen 22 atoms. The pure ligneous structure found within the cells of plants, was found to be composed of carbon 36, hydrogen 24, oxygen 24. Now, as water is a compound of 1 atom of hydrogen and 1 atom oxygen, the forms of vegetable matter may be regarded in the light of compounds of carbon with variable proportions of water; oak wood being a compound of 36 atoms of car-

bon with 22 atoms of water. There can be no doubt, however, that the elements are intimately united with each other, and do not exist as mere carbon and water; still this extreme simplicity of constitution was designed for an allwise purpose, and greatly facilitates the necessary changes which must take place on the death of an organic body.

When such a body dies, and is exposed to the action of the air, it is probable that 2 atoms of the atmospheric oxygen unite with 2 atoms of the hydrogen of the plant, and form 2 atoms of water, which are eliminated, whilst 2 atoms of the oxygen of the organic body being thereby liberated, immediately unite with one atom of the carbon of the plant, and form one atom of carbonic acid, which is also eliminated; so that, for every atom of carbon removed, 2 atoms of hydrogen and 2 atoms of oxygen are also displaced, and in time nothing but carbon would remain, were the process to go on in the atmosphere freely and undisturbed. When the process is about half completed, what is called vegetable mould, or humus, is formed.

When a vegetable body decomposes under water, the circumstances in which it is placed being different to its condition when freely exposed to the atmosphere, the process and order of decomposition are also very different. It decomposes with limited access of air, and in contact with a body (water) itself susceptible of decomposition in the presence of decomposing organic matter. Water has the property of absorbing a certain quantity of oxygen from the atmosphere. 100 cubic inches of water at a temperature of 60° Faht., and under a pressure of 30 inches mercury, will absorb about 3½ cubic inches of oxygen. It is well known that all plants in a living state absorb oxygen united to carbon or carbonic acid; and hence it is, that the herbage on the brow of a hill, on which a rill of water trickles, looks so bright and green, and fresh and vivid, from the incessant supply of oxygen to its roots, in the best form for oxidizing

the carbon of the manure, and supplying it with the carbonic acid which constitutes its chief food. And hence it is, that in stagnant water, the oxygen soon being removed by the immersed plants, and a farther supply from the atmosphere being prevented by the still water, the herbage is dark and rank, sickly and unpalatable.

We have seen, that by the absorbed oxygen of the water, and we may add, by the action of the wind and waves, dead plants, immersed in water, are not entirely, at least at first, excluded from contact with oxygen, but that this is supplied in very limited quantity. When the plants begin to decompose, the water participates in the change, its elements unite with the decomposing matter, and the oxygen which it held in solution is absorbed by the decaying vegetable matter, carbonic acid being given off. It has been stated above, that the composition of wood may be represented by carbon 36; hydrogen 22; oxygen 22. This is the actual composition of perfectly purified, well-dried oak wood, as determined by Gay Lussac and Thenard; and although different varieties of wood may afford minute deviations from these proportions, yet it may be assumed that these represent the empirical formula, and may be taken without much risk of error as the groundwork of our reasoning and calculations. When oak wood is decomposed under water, a white mouldered matter is formed, which yields on analysis, carbon 33, hydrogen 27, oxygen 24. Now, if to the elements of oak wood, carbon 36, hydrogen 22, oxygen 22, we add the elements of 5 atoms of water, with 3 atoms of oxygen = hydrogen 5, oxygen 5 + oxygen 3, and subtract 3 atoms carbonic acid, carbon 3, oxygen 6, we have the exact composition of the altered wood, or mouldered oak. And this is what must really take place; 5 atoms of water or its elements, and 3 atoms of oxygen from the water and air, unite with the decomposing woody matter, and 3 atoms of carbonic acid are given off and escape.

Whenever free oxygen, as that of the atmosphere, has access to the decomposing matter, it is probable that no hydrogen is removed from the plants, except in union with it, and therefore in the form of water. In the change of wood into wood coal, in which the whole ligneous structure is preserved, this would appear to have been pretty constantly the case; for in one specimen of wood coal analysed in Liebig's laboratory, the composition differed from oak wood merely in the loss of 3 atoms of carbonic acid and 1 atom of hydrogen; and in another, which had undergone farther decomposition, by 4 atoms carbonic acid, 5 atoms water, and 2 atoms hydrogen. It is most probable that in both these cases the hydrogen had been removed by free oxygen, and the carbon by the oxygen of decomposed water. A beautiful illustration of the formation of wood coal, by the united action of water, and a limited supply of air, accelerated by a high temperature, was afforded in the analysis of a piece of wood, which had been long kept in the boiler of a steam-engine, and had acquired the appearance of wood coal. It had exactly the composition of the first of the wood coals spoken of above, viz., carbon 33, hydrogen 21, oxygen 16, having lost 3 atoms of carbonic acid, and 1 atom hydrogen. When vegetable matter decomposes under an entire exclusion of air, or nearly so, as must take place in deep or still water, or when imbedded in such masses of rock as we find the coal formation to be, the changes, no longer influenced by free oxygen, must vary from those already described; and some products of a different character be eliminated as the result of the decomposition. The carbon, still seeking oxygen from every available source, obtains some from the decomposition of the water in contact with the decaying vegetable matter; some from the vegetable itself, whose relative quantity is continually diminishing; and a portion from the salts originally existing within the decaying mass, and in the

water in which these changes take place, reducing the sulphates, phosphates, &c., which chiefly constitute them, to sulphurets, phosphurets, &c. The metallic bases with which these are ultimately left in combination, being finally oxidized by the oxygen of decomposed water, unite with the carbonic acid eliminated by the decaying matter, forming carbonates of the earths, alkalies, &c., which originally existed in other forms in the vegetable fabrics, and in the water. The hydrogen, liberated from the decomposed water, seizes on the sulphur, phosphorus, and carbon, with which it is in contact at the moment of liberation, and escapes as sulphuretted hydrogen, phosphuretted hydrogen, and carburetted hydrogen. A portion of the hydrogen also of the decomposing plants now enters into new combinations; part uniting with the oxygen of the plant, forming water, and another part with its carbon, forming light carburetted hydrogen, or marsh gas, or fire damp, a gas composed of 2 atoms of hydrogen, and 1 atom of carbon, or of carbon 2, hydrogen 4. Instead of the mere elimination of carbonic acid and water, then, as takes place when vegetable matter decomposes or decays with free access of air, we have, when occurring under water, or in contact with water, with exclusion of air, the formation and escape of carbonic acid, light carburetted hydrogen, sulphuretted hydrogen, phosphuretted hydrogen, which are all gaseous, and water, which remains behind. Whoever has stood over a marsh or a stagnant pool, or watched a foul drain drag its slow length along, has observed bubbles of gas to gurgle up to the surface, float awhile and burst. The gas contained in these bubbles, on analysis is found to consist of carbonic acid, light carburetted hydrogen (hence called marsh gas), and, when in considerable quantity, sulphuretted hydrogen also.

The pale phosphoric light which seems to enwrap masses of decaying wood in the interior of trees, sometimes called

phosfire, is due to phosphuretted hydrogen. In tropical countries, favoured by the warmth, the succulent vegetation brought down by the large rivers into the stiller waters of their estuaries, together with the abundant maritime vegetation naturally growing therein, decompose with a rapidity unknown in our colder climes, and pour off their gases in immense volumes, creating a pestiferous stench, and a destructive miasm, fearfully fatal to the adventurous European who may visit these fetid waters.

The production of carbonic acid and light carburetted hydrogen must often be sufficiently simple; for 2 atoms of carbon uniting with the element of 2 atoms of water containing $O_2$ $H_2$, would produce 1 atom carbonic acid $C.\ O_2$, and 1 atom light carburetted hydrogen $C.\ H_2$. It will not be necessary, in the present state of geological knowledge, to adduce any proofs of the vegetable origin of coal and cannel. Independently of the occurrence of the trunks of trees within the beds, and the abundant existence of fern-like organic remains in the roofs and floors of the coal seams, thin slices of coal, when examined under a microscope, exhibit a true ligneous cellular structure, and have all the appearance of wood. The very frequent occurrence, not only of the scattered teeth, bones, and scales of fish, but of their entire skeletons, in cannel, show that this latter was either formed under water, as described above, or was submerged very soon after its formation. The great extent to which the original structure has been destroyed in cannel, also points out the subaqueous formation of it.

The remains of fish are never found in ordinary coal, and the ligneous structure is much better preserved. As the accumulation of vegetable matter, by changes on the earth's surface, became covered with mineral deposits, and air and even water more effectually excluded from the changing mass, its own substance was compelled to furnish the oxygen to the hydrogen and carbon of the decaying plant, and thus

in more rapid and unequal proportion, was this diminished in the forming coal. It has been shown by the products of the decomposition of vegetable matter and water, when air is excluded, that the elements of the decaying matter divide themselves amongst each other, so to speak, under these circumstances, and pass off; one in combination with a portion of each of the rest, the proportion of the combination, as will be shown hereafter, varying with the temperature. In the earlier periods of decomposition and entombment, the carbonic acid evolved would be large, relatively to the carburetted hydrogen and water; but as the process went on, and the oxygen became diminished in the decomposing matter, these proportions would become reversed, till scarcely any thing but carburetted hydrogen would be at length given off by the coal, and this even would finally cease on its conversion into anthracite. Let us see how far analysis will carry out this reasoning. There is no reason for believing, however much the external form, and perhaps internal mechanical structure, of the Flora of the ancient world may have differed from that of the present, that the composition of the woody fibre of that remote period differed in any material degree from its composition now. Prodigal as nature is in shapes, and forms, and hues; unsparingly as arrangements have been varied—the Almighty hand that guides her operations works with the simplest means, and the most unchanging processes. His power displays itself in the infinite variety accomplished with the most limited materials. It is reasonable to suppose, it is in accordance with all knowledge of the subject, that the same formula that represents the composition of woody matter now, would exactly correspond with, or closely approximate to, that representing its composition in the vegetation of a past world. We have seen before, that the general formula for woody fibre now may be represented by $C_{36}\ H_{22}\ O_{22}$. An analysis of the cannel coal of Lancashire, and of the splint coal of New-

castle, gave the formula $C_{24}$ $H_{13}$ $O_{1}$. The analysis of coal from the Oakwell Gate colliery, near Gateshead, and from the Hebburn colliery, corresponds very nearly to this formula. Of course the greater number of coals and cannels will vary from it more or less, but it may be taken as the expression of a general formula.

| | | | | |
|---|---|---|---|---|
| Now 1 atom of wood.............. | C. 36 | H. 22 | O. 22 | |
| Minus 9 atoms carbonic acid...... | C. 9 | | O. 18 | |
| „ 3 atoms water.............. | | H. 3 | O. 3 | |
| „ 3 atoms carburetted hydr. | C. 3 | H. 6 | | |
| = | C. 24, | H. 13, | O. | the composition of coal. |

So that all the elements of the vegetable fibre have participated in the decomposition; and the wood, in its conversion into coal, has evolved from its structure 9 atoms carbonic acid, 3 atoms water, and 3 atoms light carburetted hydrogen. It would appear, that in the earlier stages of decomposition, when the air had partial access to the decaying matter, and oxygen existed in the mass as a main constituent, that the carbon and hydrogen combined with this agent, in preference to uniting with each other, as might, from the immense combining or chemical energy of oxygen, have been anticipated, and that thus, during these periods, only water and carbonic acid would be evolved, or with a very minute proportion of carburetted hydrogen. The wood coal or brown coal of Lavbach in Hesse-Darmstadt is composed of C. 33, H. 21, O. 16, and differs from fresh wood, therefore, by the elements of 3 atoms carbonic acid and 1 atom of hydrogen. The wood coal (brown coal) of Ring Kuhl, near Cassel, is much further decomposed, and is losing the woody structure. It contains C. 32, H. 15, O. 9, and differs from wood by the loss of 4 atoms carbonic acid, 5 atoms water, and two atoms hydrogen. There can be no doubt that the surplus hydrogen has been removed in both these cases by external oxidation. The gas eliminated in mines of wood coal is invariably carbonic acid, and never contains carbu-

retted hydrogen. The avidity with which the decomposing vegetable mass seizes, even when converted into beds of coal, on every available source of oxygen, was lately observed in analysing the gas (consisting chiefly of fire-damp) from the mines of Newcastle; the nitrogen forming from 14 to 21 per cent., whilst scarcely any oxygen remained. Now, the nitrogen must have been derived from atmospheric air, admitted by the mine to the coal, which had removed the oxygen and combined with it. The constant presence of sulphuret of iron in coal, which originally must have existed as sulphate of oxide of iron, and been converted by the removal of its oxygen into sulphuret, also shows the powerful deoxidizing power of the decomposing organic mass. When, after a long lapse of ages, the oxygen had been gradually removed from the vegetable mass, in the form of carbonic acid and water, until at length the wood coal had lost its structure, and acquired the composition possessed by our own beds of more perfectly formed coal, in which 1 atom of oxygen only remains in union with 24 atoms of carbon and 13 atoms hydrogen, or approached this composition, it is evident that a new series of results must attend the changes going on within the still altering coals; oxygen no longer existing for the formation of carbonic acid, the carbon and hydrogen now constituting almost the entire mass of the coal, must of necessity unite, and escape as carburetted hydrogen. An analysis of the gas evolved in mines from coal, shows it to consist almost exclusively of light carburetted hydrogen. The following analysis by Mr. Wrightson, made in the laboratory of the Museum of Economic Geology, of the gas evolved from a seam in the Hebburn colliery, will show this:—

| | |
|---|---|
| Light carburetted hydrogen | 91·8 |
| Carbonic acid | 0·7 |
| Nitrogen | 6·7 |
| Oxygen | 0·9 |
| | 100·1 |

There can be no doubt, that the minute quantity of carbonic acid present, had been formed by the atmospheric oxygen, the large excess of nitrogen showing that the air had been robbed of its oxygen; whilst it is further evident, that the coal itself was pouring out from its own materials pure carburetted hydrogen. As light carburetted hydrogen contains 2 atoms of hydrogen to 1 atom of carbon; it is also evident, that our present beds of coal are hastening to the condition of anthracite, which consists almost entirely of carbon, and has gone through all the stages of forest, peat, and wood coal, to anthracite. Cannel appears to differ from coal in having been formed under water; its abundant remains of fish, interspersed through its substance, its layers of sulphate of lime, which it could have obtained from no other source, and which are not found in coal, all prove this; whilst its conchoidal fracture and homogeneous texture, seem to indicate that it formerly existed in a softened muddy state. Where the temperature has been low, we have no evidence of the formation of any higher carburets of hydrogen than that of fire-damp, which contains 1 atom of carbon to 2 atoms of hydrogen, during the decomposition which precedes the formation of coal; but under an elevated temperature, such as could be produced by an injection of ignited matter into the adjacent super or sub jacent rocks, we have new affinities called forth, and compounds formed, in which the relation of carbon and hydrogen is altogether different, the atoms of carbon sometimes exceeding those of hydrogen in the compounds, and sometimes being of equal number. We know that no such injection of heated matter now takes place within the limits of the Lancashire coal-field, or the coal-fields of Northumberland and Durham; and in the fire-damp of the mines sunk there, we find no higher carburet of hydrogen than that so often spoken of. But where the coal-measures have been traversed by dykes of trap rock, which must have been

injected in a melted state, the neighbouring coal has been subjected to a true distillation, and products are found in the vicinity, the ordinary results of such action. Thus, in Derbyshire, where the measures have been traversed by dykes of trap, popularly called toadstone, springs of naphtha are found, which must have distilled from the coal. Similar springs are found at Baku, near the Caspian; at Ammiano in Italy, at Rangoon, and in some parts of Germany, &c. The analysis of the fire-damp which streamed out of clefts in the coal at Wallesweille, Luisenthal, and Lickwey, indicated the presence of from 6 to 16 per cent. of olefiant gas, according to Bischoff. Olefiant gas contains 2 volumes of carbon and 2 of hydrogen, condensed into 1 volume. In some places, when the coal has been near to the heated matter, it has been found completely charred, and converted into coke.

In reflecting on these decompositions, there are two circumstances that strike us as remarkable, and which possess peculiar significance. The first and most remarkable fact, is the entire absence of pure hydrogen in any of the gases evolved by the decomposing vegetable matter, or in any of the fire-damps issuing from the decomposing coal, although so constantly present in coal gas. The second is the equal absence of olefiant gas, or of any other compound of carbon and hydrogen, except the light carburetted hydrogen, C. H. 2, unless under circumstances that could lead us to believe that the coal had been subjected to a high temperature, and that the higher carburets of hydrogen were true products of distillation, where olefiant gas, naphtha, petroleum, &c., are found as natural products. It will be apparent, then, if the foregoing reasonings and remarks be admitted as proof, that it is a law of nature, that when organic masses decompose without access of air—that is, with exclusion of free oxygen—all the elements participate in the change, and unite reciprocally with each other; the

mode of union, and the products of that union, being regulated by the temperature. That, including in the consideration the nitrogen, which, though not a constituent of the woody tissue, is invariably found in the juices and in many of the organs of a plant, and permeates every part of it, when vegetables decompose with free access of air, the products are carbonic acid, water, and ammonia; when under water, with very limited access, or total exclusion of air, carbonic acid, water, carburetted hydrogen, ammonia, sulphuretted hydrogen, phosphuretted hydrogen, &c.; with limited supply of water, and exclusion of air, as in wood coal, carbonic acid, water, and a little carburetted hydrogen, till the process having nearly exhausted the oxygen, as in truly fossilized coal, the carburetted hydrogen exclusively takes the place of the carbonic acid; and finally, that when the divellent affinities are exalted by a high temperature, other compounds are formed, in which some of the elements are united with each other in increased proportions, (olefiant gas, naphtha, petroleum, &c.) It is worthy of remark, that when olefiant gas is found in the fire-damp of mines, when the coal is supposed to have been subjected to heat, its proportion varies from 1·5 to 16 per cent. The most usual proportion was about 6 per cent. These numbers represent the whole amount of illuminating gases existing in the gas here formed by nature's operations. The analyst is Bischoff; and nowhere but in Germany has this gas hitherto been found in fire-damp. When vegetable bodies, or bodies of vegetable origin, as coal, cannel, &c., are subjected to distillation in close vessels, without access of air, as in the process of gas-making, manufacture of pyroligneous acid, &c., the products will vary with the composition of the substance employed, and with the temperature. We have seen that all the elements participate in the change, and form new combinations. The proportion and relation of the elements of fresh wood, and of coal,

being then unlike, the results of the application of heat or other agents must also be unlike, analogous but yet unlike.

Both give off gases, but those of coal are richer in carbon; both give off oils, but those of coal are richer in carbon. Naphtha is the turpentine of coal. The products of coal are alkaline; those of wood, acid, arising from the large relative quantity of oxygen that the latter contains (C. 36, H. 22, O. 22, being the composition of wood); C. 24, H. 13, O. being that of coal. The oxygen in wood seizes the hydrogen, and diminishes the production of illuminating gases—the gases found consisting of carbonic acid, light carburetted hydrogen, and very little olefiant gas; the oxygen also seizes on the combined hydrogen and carbon, forming acetic acid, a compound of C. 4, H. 3, O. 3, pyroxilic spirit (wood naphtha, wood spirit), a kind of alcohol containing (C. 2, H. 4, O. 2), xylite (C. 12, H. 12, O. 5), another liquid (C. 21, H. 23, O. 10); all compounds containing a large amount of oxygen, and from which it will be seen how important a part the large amount of this element contained in wood plays in the products of its destruction, and modifies the results of its distillation. The gases from wood and from peat, as well as from brown coal (wood coal), possess a very low illuminating power. Coal is a compound of carbon, hydrogen, a little oxygen, very little nitrogen, earthy constituents constituting its ashes, sulphur in the form of sulphuret of iron (iron pyrites), and in cannel occasional layers of sulphate and carbonate of lime. Its empirical formula is C. 34, H. 13, O. When distilled at a high temperature in close vessels, part of the hydrogen unites with carbon, forming light carburetted hydrogen, olefiant gas, gaseous hydro-carbons, naphtha, and its associated oils. Another part unites with oxygen, forming water; another with nitrogen, forming ammonia; a fourth with sulphur, forming sulphuretted hydrogen; and a fifth with cyanogen, forming prussic acid. Of the carbon, part unites with hydro-

gen as above; part with oxygen, forming carbonic acid and carbonic oxide; part with nitrogen, forming cyanogen; part with sulphur, forming sulphuret of carbon. So that there is a perfect division of the elements amongst each other; and it can never be that the whole of the hydrogen shall unite with the carbon, and produce illuminating gases only. Here, however, we have no actual or necessary production of hydrogen, whose presence in the gas, therefore, must be the result of a defect in the process of gas-making. A little chlorine also is present in most cannels in the form of chlorine salts, and is given off as muriatic acid, but always in combination with the ammonia. A little sulphurous acid is likewise found in combination with the ammonia. The muriatic and sulphurous acids never pass into the gas holder, and need not be considered, being condensed with the ammonia. The cyanogen, sulphuretted hydrogen, ammonia, and carbonic acid, constituting a very minute proportion of the whole gas, ought to be all removed in the process of purification; so that there remain as constant ingredients of the gas, as at present manufactured, hydrogen, light carburetted hydrogen, olefiant gas, volatile hydro-carbons, carbonic oxide, and a little nitrogen; and of these, the olefiant gas, volatile hydro-carbons, and light carburetted hydrogen, alone contribute to illumination. The water formed and condensed from the distillation (gas water, ammonia water), retains in solution carbonate, sulphite, muriate, hydro-sulphate, and prussiate of ammonia; the tar which condenses from the distillation, consists of numerous oils, called naphtha, heavy oil of tar, &c., composed almost entirely of carbon and hydrogen.

I add here a tabular view of the products of the distillation of coal, with the composition of each.

### 1. GASEOUS.

| | |
|---|---|
| Hydrogen | H. |
| Light Carburetted Hydrogen | H. 4 C. 2 |
| Olefiant Gas | H. 4 C. 4 |
| Volatile Hydro-carbon | H. 6 C. 6 probably |
| Benzole | C. 12 H. 6 |
| Carbonic Oxide | C. O. |
| Cyanogen | C. 2 N. |
| Sulphuretted Hydrogen | H. S. |
| Ammonia | H. 3 N. |
| Aqueous Vapour | H. O. |
| Sulphurous Acid | S. O. 2 |
| Hydrochloric Acid | H. Cl. |
| Carbonic Acid | C. O. 2 |
| Sulphuret of Carbon | C. S. 2 |
| Nitrogen | N. |

### 2. AQUEOUS.

Water holding in solution

Carbonate of Ammonia.
Hydro-sulphate of Ammonia.
Prussiate of Ammonia.
Sulphate of Ammonia.
Muriate of Ammonia.

### 3. OILY.

| | | | | |
|---|---|---|---|---|
| Liquid | Benzole | Neutral | C. 12 H. 6 | Constituents of Naphtha. |
| | Toluol | | C. 14 H. 8 | |
| | Cumol | | C. 18 H. 12 | |

| | | | |
|---|---|---|---|
| Liquid | Aniline | C. 12 H. 7 N. | Constituents of heavy oil of tar. |
| | Picoline | C. 12 H. 7 N. | |
| | Leucoline | C. 18 H. 8 N. | |
| | Hydrate of Phenyle | C. 12 H. 6 O. 2 acid. | |
| Solid | Naphthaline | C. 20 H. 8 | |
| | Paranaphthaline | C. 30 H. 12 | |
| | Pyren | C. 15 H. 3 | |
| | Chrysen | C. 12 H. 4 | |

Various undescribed oils.

Of these oily products, which are all contained in the tar, it will be perceived that only one contains oxygen, and this possesses acid properties. The first three are neutral,

and constitute rectified coal naphtha. Three are alkaline, and contain nitrogen. The other four are solid and neutral. It is worthy of remark, how few of the products of distillation contain any oxygen, and how much they differ in this respect from the products of distillation of wood; and that, where the oxygen does enter into combination, it produces compounds having no illuminating properties, viz., carbonic oxide, carbonic acid, and water; and that in one instance it unites with a compound of carbon and hydrogen, producing an acid oil, that is found in very small quantity in the tar.

The nitrogen of the coal forms ammonia, cyanogen, and a few alkaline oils, the latter found in small quantity in the tar. The products are nearly all compounds of carbon and hydrogen, and respecting these it is further worthy of remark, that when the hydrogen exists in the compound in greater quantity than the carbon, as in light carburetted hydrogen, which contains 2 atoms hydrogen to 1 of carbon, the compound is permanently gaseous; this gas has been subjected to a pressure of 32 atmospheres, and to cold 166 degrees below zero, without liquefying. Olefiant gas, in which carbon and hydrogen exist in equal proportions, but in which 2 volumes of hydrogen and 2 of carbon are condensed into one volume, is permanently gaseous at the ordinary atmospheric temperature and pressure, but becomes liquid under a pressure of 27 atmospheres at zero of Fahrenheit.

The volatile hydro-carbons in coal gas, the exact nature of which has not yet been determined, and whose composition is valuable, or rather, perhaps, whose proportions in the mixture are valuable, probably consist of propylene $C_6\,H_6$, Faraday's gas, $C_8\,H_8$, and benzole, $C_{12}\,H_6$, with perhaps a portion of Mansfield's allyle. In my earlier experiments on coal gas, which had been made from a different cannel to what is now employed at the Manchester Gas-Works, I found, pretty uniformly, that each volume of the gas condensible by sulphuric acid or chlorine re-

quired $4\frac{1}{2}$ volumes of oxygen for combustion; subsequently I found that, when richer cannels were used, the condensible gases required a still larger proportion of oxygen for combustion. The fact, that a greater amount of carbonic acid is produced on exploding the gas with oxygen, than would proceed from a body of the series $C_n H_n$, shows that some other hydro-carbon, the carbon in which exists in a greater ratio to the hydrogen than in this series, is to be found in coal gas.

When the number of atoms of carbon, in a compound, exceed those of the hydrogen, they are, within certain limits, liquid; beyond those limits, solid; thus benzole—carbon 12, hydrogen 6, in which the carbon is double the hydrogen; toluol—carbon 14, hydrogen 8; and cumol—carbon 18, hydrogen 12, are liquid. Whilst naphthaline—carbon 20, hydrogen 8; paranaphthaline—carbon 30, hydrogen 12; pyren—carbon 15, hydrogen 3; and chrysen—carbon 12, hydrogen 4, are solid. It may be safely asserted, that where the carbon and hydrogen exist in an equal number of atoms, the compound will be gaseous, unless a very large number enter into the combination.

Let us now see what are the practical inferences to be drawn from the preceding postulatory statements. In the first place it will be manifest, that the greater the quantity of hydrogen, and the less oxygen and sulphur a cannel or coal may contain, the better it will be for gas-making; for the two latter rob the coal of a portion of its hydrogen, which is thereby prevented from uniting with a portion of carbon for the production of an illuminating gas.

The coal should be selected as free from iron pyrites and sulphate of lime as possible, and lumps or masses of these should be thrown out, as they often occur in such a form in the coal. The coal should be moderately dry before being used, which can only be secured by being stacked under cover, otherwise the rain would keep it saturated with

moisture. The water, in its decomposition in the retorts, furnishes oxygen to the carbon of the coal, impoverishing the gas, whilst the hydrogen of the water does not combine with the carbon of the coal, but is liberated in the simple state.

When the vapour of water is passed over red-hot coke and coal, and analysed, the resulting gas is found to consist in 100 volumes, of hydrogen 56, of carbonic oxide 29, carbonic acid 15·8, and of light carburetted hydrogen only two-hundredths of 1 per cent.

It contains no olefiant gas whatever; this experiment is quite conclusive against the use of water or steam. It is evident that there are no products of the decomposition of water by red-hot coal or coke that possess any illuminating power. It has often been proposed to pass steam into the retorts during the distillation of coal, but such a proceeding could have no good effect, but the contrary. When it is considered that 50 per cent. of the whole of the gases proceeding from the decomposition of water by red-hot carbonaceous matter is hydrogen, another very formidable objection arises to its use; viz., that it would not only diminish the light of gas with which it was mixed, but would give out such an amount of heat during the burning of it, as would render the use of such gas almost insupportable. The coal then should be dry; but we have also seen that air, passed between seams of coal, has been deprived of a portion of its oxygen, which must have combined with the carbon and hydrogen of the coal, and by as much have impaired its quality. We have seen that coal and cannel are continually giving off gas (fire-damp), and this teaches us that the coal should be dried quickly and then used.

Let us now consider what takes place during the manufacture of gas by the distillation of coal in red-hot retorts. The nitrogen in gas is entirely derived from atmospheric air, admitted into the retorts during the charges, and by

leakage in the apparatus, and is not a product of the decomposition of the coal at all; it need not, therefore, enter into our consideration.

The quality and illuminating power of the gas will be affected, not only by the quality (composition) and condition (wet or dry, old or recently got) of the coal or cannel, but by the degree of heat employed in its preparation, and the mode in which the operation is conducted. The chief products of the distillation are compounds of carbon and hydrogen, and these alone yield light; but of these we find that some are solids, some liquids, and some gaseous—the two first are valueless for the purpose of illumination, because their physical condition (solid and liquid) precludes their use. The gaseous are three—one containing very little carbon (light carburetted hydrogen), and, therefore, giving very poor light; the other two very rich in carbon (olefiant gas—carbon 4, hydrogen 4; and volatile hydro-carbon—carbon 6, hydrogen 6, or carbon 8, hydrogen 8, &c.), and giving great light, though in small quantity. We find mixed with these, besides the necessary impurities (sulphuretted hydrogen, ammonia, carbonic acid, &c.), two gases constituting the chief bulk of the mixed coal gas, which we have also seen are never given off in natural operations, viz., hydrogen and carbonic oxide. They are also not necessary products of the distillation, but result from the mode of distillation.

The carburets of hydrogen may be conveniently divided into three classes; in the first, the number of atoms exceeds that of the hydrogen—they are very rich in carbon, as benzole (carbon 12, hydrogen 6); these are either liquid or solid, and would give great light could they be burnt, but give great smoke;—in the second, the atoms of carbon and hydrogen are equal in the compounds, as olefiant gas (carbon 4, hydrogen 4), volatile hydro-carbon (carbon 4, hydrogen 6, and carbon 8, hydrogen 8); these are gaseous,

but condensible by great pressure and intense cold, and give much light;—in the third, the atoms of hydrogen exceed those of the carbon; these are altogether uncondensible, and give little light (carbon 2, hydrogen 4). When coal and similar organic matters are distilled at a comparatively low temperature, the carbon has a disposition to pass off with little hydrogen; the liquid hydro-carbons are formed, there is much tar and little gas, but the gas is rich. As the temperature rises, the liquid hydro-carbon diminishes in quantity, and gaseous hydro-carbon increases; there is more gas and less tar (olefiant gas and volatile hydro-carbons). The temperature still rising, the gaseous products become richer in hydrogen, and poorer in carbon; light carburetted hydrogen is formed in abundance; and at length, the temperature becoming still higher, pure hydrogen is given off, as is always observed in the last hour's distillation in gas-making.

It is a well-known law of organic chemistry, that the higher the temperature, and the more advanced the decomposition of organic matter, the simpler are the products.

When olefiant gas is passed through red-hot tubes, or over red-hot lime or crystal, or, in fact, over any red-hot surface, it deposits a portion of carbon on the red-hot matter in a solid form, and escapes as a mixture of carburetted hydrogen, and hydrogen. The same thing I have proved also of naphtha, C. 12, H. 6, which deposits carbons in like circumstances, and is resolved into simple products.

The affinity between carbon and hydrogen seems to diminish with the temperature.

Is it not probable, that in the distillation of masses of coal, compounds rich in carbon are first formed, the carbon being in excess of the hydrogen; as the product rises in temperature, it deposits a portion of its carbon, the atoms of hydrogen become equal, and a rich gas is formed; but this, getting still hotter, deposits more carbon, the hydrogen

is now in excess, the gas is poor and gives little light; the heat still increasing, the affinity between the hydrogen and carbon is altogether disrupted, the remaining carbon is deposited, and pure hydrogen given off? Certainly all this can be effected artificially; and that, to a large extent, it is so in gas-making, is evident from the thick lining of almost pure carbon which soon forms in the interior of gas retorts, and which must proceed from the decomposition of the gas by the red-hot surface—must be deposited from it, in fact. Still it is not simply and entirely thus; there are probably three products, at least, of the decomposition of a liquid carbo-hydrogen, solid carbon, a gaseous product containing much hydrogen, and a solid hydro-carbon containing much carbon, the elements being divided amongst each other. When naphtha vapour is passed over red-hot crystal, it deposits carbon, gives off olefiant gas and light carburetted hydrogen, and forms a crystalline compound, naphthaline, composed of C. 20, H. 8. With these facts before us, is it not reasonable to conclude, that there is a temperature, a point at which, in the process of decomposition, olefiant gas and volatile hydro-carbon (which I would call trito or tetarto carburet of hydrogen) should be formed, and which yet should be unable to decompose these into compounds poorer in carbon? for we have seen that the intensity of decomposition is proportionate to the intensity of heat. I think there cannot be a doubt that there is such a temperature; but it must be far below that at present employed for the manufacture of gas. Let us now examine the present system of gas-making, and I think we shall soon see the true source of the hydrogen and carbonic oxide, so invariably found in gas, and constituting so large a portion of its bulk. I may premise, that when carbonic acid is passed over red-hot coke it is resolved into carbonic oxide, by taking up an additional atom of carbon.

When compact masses of coal are thrown in heaps of a

hundred-weight and a half, into retorts heated into a bright redness as is now done, it is exposed to two very different conditions; the surface of the mass, the exterior, in contact with the intensely hot retort is instantly decomposed, and charred; hydro-carbons, as olefiant gas, &c., are eliminated, which also, at this high temperature, are partly decomposed, and resolved into light carburetted hydrogen and pure hydrogen, with deposition of carbon, which, with some undecomposed olefiant gas and volatile hydro-carbons, pass off from the retort—the interior of the mass, on the contrary, is for some time exposed to a very moderate temperature, and a simple distillation is accomplished; those compounds which are formed at a comparatively low temperature, the heavy hydro-carbons, which would ordinarily be in a liquid state, are given off; a portion, rising into vapour as it reaches the hotter surface, passes off with the gases formed, and condenses again when it has left the retort in the form of tar; but that portion of the vapour which, in its passage, comes into contact with the red-hot surface of the exterior of the mass and of the sides of the retort, deposits a portion of its carbon, and is resolved into simple compounds, olefiant gas and volatile hydro-carbons, which themselves partly undergo the change already described. As the heat penetrates to the centre, and a red-hot mass of charred material of considerable thickness comes to surround the decomposing coal within, as happens towards the end of the distillation, the whole of the hydro-carbons, viz., light oils, volatile hydrocarbons, olefiant gas, and even light carburetted hydrogen itself, that are eliminated, are decomposed in passing over such an extent of red-hot surface, and pure hydrogen is almost alone evolved. The carbonic oxide, which is formed from the union of the oxygen of the coal and of the air admitted with the carbon of the coal, is also partially decomposed during the whole of the process, but in an opposite direction; not by depositing carbon, but by taking up more, and being

converted into carbonic oxide, which is evolved with the gas. These are the true sources of the hydrogen and carbonic oxide in gas; they are not necessary results of the distillation, but products of the decomposition of the distilled matter. It has been perfectly ascertained by myself, and by other chemists, that when olefiant gas is passed through a nearly white hot porcelain tube, it is entirely decomposed, depositing the whole of its carbon, and giving off pure hydrogen gas. The late Dr. Henry subjected cannel coal to distillation, beginning with a moderate heat, and gradually raising it—the degree of heat is not specified, but it was much inferior to that generally employed in gas manufacture.

The operation lasted 10 hours, and he examined the gas at the beginning of the process, after 5 hours, and after 10 hours. At the beginning of the process, 100 parts of the gas contained 13 of olefiant gas, 82 of light carburetted hydrogen, 3 of carbonic oxide, and the rest nitrogen and hydrogen. After 5 hours, 100 parts contained 7 olefiant gas, 56 light carburetted hydrogen, 11 carbonic oxide, and 21 hydrogen. After 10 hours, the gas contained no olefiant gas, 20 light carburetted hydrogen, 10 carbonic oxide, and 60 hydrogen. With these analyses my own entirely accord, except that, from the greater heat employed, I obtained hydrogen and carbonic oxide almost from the beginning. The decomposed matters occupy greater bulk than the original substances from whose decomposition they proceed. The greater the number of atoms of carbon and hydrogen combined together the less space they occupy (C. 20, H. 8, is a solid naphthaline); C. 12, H. 6, is a liquid naphtha; C. 4, H. 4, is a gas (olefiant gas). This gas, on depositing a portion of its carbon, becoming C. 2, H. 4 (light carburetted hydrogen), retained its original bulk, which latter gas is, therefore, more voluminous for its composition than olefiant gas. Were a gas containing 1 atom carbon and 1 atom

hydrogen known, which at present is not, it would undoubtedly occupy the space of olefiant gas. In other words, could olefiant gas, C. 4, H. 4, be resolved into 2 atoms of C. 2, H. 2, it would occupy double the space of the olefiant gas itself. For olefiant gas itself occupies exactly the space of the volatile hydro-carbon, C. 8, H. 8; and it is ascertained, as stated before, that when 1 volume of olefiant gas is passed through a nearly white hot porcelain tube, it is certainly decomposed, depositing all its carbon, and giving 2 volumes of hydrogen. In other words, its bulk is doubled by the decomposition; and 1 volume of the volatile hydro-carbon, C. 8, H. 8, on being decomposed by heat, and depositing carbon, forms 1 volume olefiant gas and 1 volume light carburetted hydrogen = to 2 volumes.

One volume carbonic acid, on becoming converted into carbonic oxide, occupies two volumes.

The greater the heat employed, then, in the process of gas-making, above a certain limit—viz., that requisite for the decomposition of the liquid hydro-carbons—the greater will be the bulk of the gas, and the poorer its quality; the more light carburetted hydrogen, hydrogen, and carbonic oxide it will contain, and the less volatile hydro-carbon and olefiant gas. The analysis of the gas will therefore furnish a test of the excellence of the process employed in the manufacture, and a check on the workman, by exhibiting, in the relative amounts of hydrogen, and of the illuminating hydro-carbon, whether too great a heat has been employed. A great quantity of gas may be made from coal, and very badly made. The mere amount of gas produced is no proof of the excellence of the manufacture.

Cannel yielding 11,000 feet of gas per ton, of specific gravity 600, would furnish for every 100 pounds distilled—about

| | lbs. |
|---|---|
| Gas | $22\frac{1}{4}$ |
| Tar | $8\frac{1}{2}$ |
| Ammonia water | $9\frac{1}{2}$ |
| Coke | $59\frac{3}{4}$ |
| | 100 |

These proportions will vary considerably, but still the numbers will represent a general average of produce. It is seen, from above, that considerably more than a third of the weight of the gas produced, is distilled from the cannel in the form of tar, which contains, and is almost entirely composed of, the richest carbo-hydrogen, and very little oxygen; whilst the gas, as it contains only about 45 per cent. of compounds of carbon and hydrogen, by measure amounting to about half its weight, really only contains about 11 pounds of carbo-hydrogen, and of this only about 4 pounds will be olefiant and richly illuminating gases. So that in the tar is really contained as much illuminating matter, or nearly so, as in the gas—not twice as much as would appear from the numbers; for it must be borne in mind, that in the oils composing the tar the carbon exists in much greater proportion than the hydrogen, one of the lightest, benzole, being a compound of carbon 12, hydrogen 6; naphthaline and the solid carburets being represented by carbon 20, hydrogen 8, and even higher proportions of carbon. So that, in the decomposition into illuminating gases, much of the weight must be lost in the form of deposited carbon. I think it is now tolerably apparent, that in the form of distilled matters nearly one-half of the illuminating matter derivable from coal and cannel is lost to the gas. It is probable that a perfect system of gas-making would produce from good cannel a gas containing 20 per cent. of olefiant gas, or other illuminating gases. We have seen, moreover, that a great waste of illuminating material takes place in the present system, from the actual destruction of illuminating gases when formed, by the large quantity of

red-hot material through which they are obliged to pass, and by which they deposit their carbon, and are eliminated as pure hydrogen, a gas which forms from 30 to 40 per cent. of coal gas, and is utterly useless in the illumination.

The production of hydrogen and tar are manifest evidence that the heat employed is too great at the surface of the coal, and too low in the centre of the mass.

When gas is made from resin, or from oil, the melted resin or the oil is allowed to fall in thin steamlets on a red-hot plate, or to trickle over a more extensively heated surface. The facility with which resin or oil is converted into volatile liquids, renders necessary a somewhat ample surface, in order that these liquids may be decomposed into gas.

There are some furnaces to steam-boilers in which the fuel is burnt with limited access of air, and is supplied to the furnaces by means of hoppers, the coal, in a somewhat finely divided state, falling on a revolving horizontal plate, and being by this scattered in a thin layer on the incandescent matter within.

This method of supplying the furnaces is not found to be profitable, and for a very sufficient reason: the coal, in the first instance, is simply distilled, and the gases eliminated meeting with little or no oxygen within the furnace, pass off undecomposed, and the heat that would arise from their combustion is not only lost, but the gases impinging on the bottom of the boiler reduce its temperature, and lower the tension of the steam. If no air whatever were admitted to the furnace, the coal would then be placed in exactly the same condition as the resin and oil which have been used for gas-making.

The course of the research indicated by the analysis of the subject of gas-making so far completed is obvious. These are the determination of the constituents of the coals and cannels to be employed as materials; a rigid examination of the products, gaseous, liquid, and solid, resulting from

their decomposition, with reference to their amount and their quality; an examination of the nature and bulk of the products eliminated by distillation at different temperatures; an examination of the results of distilling coal in aggregated masses and in thin layers; an examination of the relative effects of heating coal in a thin layer on a sufficiently hot surface, with a comparatively cool arch above, as in a D shaped retort, with the bottom alone heated; and the effects of surrounding the material with a heated surface, as in the round retorts heated all round.

## XIX.—*On Linear Constructions, by* Rev. Thos. Penyngton Kirkman, A.M., *Rector of Croft-with-Southworth.*

Read March 18, 1851.

It is generally known to mathematicians, and is stated by Professor Chasles in his "*Aperçu Historique, &c.,*" as well as by Professor Steiner in his "*Systematische Entwickelung, u. s. w.*" (*Anhang*), that the following question was twice proposed as a prize question by the Academy of Brussels above twenty years ago, and received no answer: "What is the relation among ten points of a surface of the second degree?" One obvious answer is, that the equation to the surface, if the constants are made functions of nine given points, expresses the required relation among the co-ordinates of ten points. But it is plain that the solution required is to be purely geometrical, and such that it shall give a criterion independent of all properly analytical results, computation of numbers, or measurement of distances, whereby it may be determined whether any tenth point lies on the surface which passes through a given nine, and where any line meets the surface a second time.

Mr. Weddle, of the Royal Military College, Sandhurst, gave a construction of the tenth point in the November number 1850 of the Cambridge and Dublin Mathematical Journal; and his is the first answer, so far as I can learn, that the Brussels prize question has received. I am not sure, nor does Mr. Weddle seem to be certain, that his solution will be accepted as purely geometrical, for it is the construction of an analytical result. Further, I am not able to say whether the Brussels Academy will grant the use of the compasses in the required construction, and Mr. Weddle does not show that his can be effected without them.

What mathematicians have been so long looking for, is a property in solid geometry like the celebrated theorem of Pascal. By this, if any five points be given in a plane, and any line through one of them, we can determine by the joining of given points, that is, with the ruler only, where the given line cuts a second time the conic which passes through the five points. Pascal's theorem requires only a ruler; but it is important to observe, that there is no limit to the length of that ruler. If the given points are 1 2 3 4 5, and A 1 be the given line, Pascal's theorem teaches us, considering the hexagon 1 2 3 4 5 6—6 being the sought point in A 1—to produce the line 34 to meet A 1, and 12 to meet 45; then, through the points (A 1, 34) and (12, 45) thus found, to draw a line cutting 23 in a point from which a line drawn through 5 will cut A 1 in the point 6 required. If now it happens, either that A 1 is parallel to 34, or that 12 is parallel to 45, the line cutting 23 cannot be drawn with a ruler of finite length, since one of its two points (A 1, 34) and (12, 45) passes off to an infinite distance. Thus, Pascal's theorem itself can be shown to fail unless an infinite ruler be conceded, that is, unless it is granted that a line is given by its direction and one of its points, or that we have the power of drawing parallels. Nor is it any answer to this claim to say, that we can choose another hexagon. For how far are we to pursue the lines A 1 and 34, in order to convince ourselves that they never meet?

Let but a ruler of unlimited length be granted me, and I will show how to effect, by its aid only, and from purely geometrical data, the solution of a more general question than the Brussels prize question, namely, this problem of linear constructions:—

*Any locus of the $N^{th}$ order (or class) being given geometrically, by the requisite number of points (or tangents) of which n-1 are in a line (or pass through a point), to find the $N^{th}$ point*

*in that line (or the $N^{th}$ tangent line or plane through that point), upon the locus, and this by the aid of the ruler only.*

By the same method can be found the value of a given *ex-local function* F $(x_1\, y_1\, z_1)$, whether $x_1\, y_1$ and $z_1$ be the co-ordinates of any assigned point, or of any given plane: *i. e.*, it can be determined whether or no $(x_1\, y_1\, z_1)$ satisfies the equation F $(x\, y\, z) = 0$.

We shall, in the first place, exhibit our data in a convenient form. Any axes and origin being chosen, we can by hypothesis draw Cartesian co-ordinates through our given points. Let us consider, by way of example, the surface of the second order. Putting $x_1\, y_1\, z_1\, x_2\, y_2\, z_2$, &c., for the co-ordinates of the nine points, 1, 2, &c., and $x_0\, y_0\, z_0$ for those of any tenth point, we shall first frame a *paradigm* of the surface, which may be compendiously represented thus—

$$\Sigma \pm x^2_0 \,.\, y^2_1 \,.\, z^2_2 \,.\, xy_{33} \,.\, yz_{44} \,.\, zx_{55} \,.\, x_6 \,.\, y_7 \,.\, z_8 \,.\, 1_9 = 0 :$$

the terms of this paradigm are in number 10 . 9 . 8 . 7 . 6 . 5 . 4 . 3 . 2, and are all formed from the first term

$$+ x^2_0 \,.\, y^2_1 \,.\, z^2_2 \,.\, xy_{33} \,.\, yz_{44} \,.\, zx_{55} \,.\, x_6 \,.\, y_7 \,.\, z_8 \,.\, 1_9.$$

by permutation of the sub-indices alone; the sign of any term being determined by the simple rule, that if it is made from the first by an odd number of transpositions of single pairs of sub-indices, it shall be negative, and positive when that number is even. Thus the terms

$$+ x^2_3 \,.\, y^2_1 \,.\, z^2_2 \,.\, xy_{00} \,.\, yz_{44} \,.\, zx_{55} \,.\, x_6 \,.\, y_7 \,.\, z_9 \,.\, 1_8$$

$$- x^2_3 \,.\, y^2_1 \,.\, z^2_2 \,.\, xy_{44} \,.\, yz_{00} \,.\, zx_{55} \,.\, x_6 \,.\, y_7 \,.\, z_9 \,.\, 1_8$$

have the signs which are prefixed to them. The cyclical permutation of an even multiplet involves always a change of sign, this being effected by an odd number of transpositions of single pairs, while that of an odd multiplet leaves the sign unchanged. The former of the two last written terms is made from the first by the cyclical permutation of two even multiplets, the quaternion 0 1 2 3, and the duad 8 9, either of which permutations *alone* would

have changed the sign; the latter is made from the former by the transposition of the pair 0 4, and it will be found deducible from the first term only by an odd number of single transpositions.

The paradigm, being formed, is either a purely geometrical datum, or an analytical one, according as we define $x_1\, y_1\, z_1$, &c., to be finite lines given in space, which we have neither the intention nor the means of measuring, or numbers, as length, in feet, of the co-ordinates. If we consider them for a moment to be numbers, the expression

$$\Sigma \pm \underset{0}{x^2} \,.\, \underset{1}{y^2} \,.\, \underset{2}{z^2} \,.\, \underset{3\,3}{xy} \,.\, \underset{4\,4}{yz} \,.\, \underset{5\,5}{zx} \,.\, \underset{6}{x} \,.\, \underset{7}{y} \,.\, \underset{8}{z} \,.\, \underset{9}{1} = 0$$

is the equation to the surface of the second order which passes through the nine points 1, 2, 3,...9; for it is plainly of the second degree in $x_0 y_0 z_0$, and it vanishes when for $x_0\, y_0\, z_0$ we put the co-ordinates of any of the nine points. For let $(x_0\, y_0\, z_0)$ and $(x_3\, y_3\, z_3)$ be the same point. The first term is now identical in value, but not in sign, with the term $-x_3^2 \,.\, y_1^2 \,.\, z_2^2 \,.\, x_0y_0 \,.\, y_4z_4 \,.\, z_5x_5 \,.\, x_6 \,.\, y_7 \,.\, z_8\, 1_9$, which being formed from the first by a transposition of the pair 0 3, stands in the paradigm with a negative sign. And thus the whole equation is reduced to a system of internecine pairs of terms.

We are thus in possession of a certain and surprisingly simple method of writing down at once the equation to a locus of any order (or class), in terms of the defining points (or tangents), provided that their number *does not exceed* that of the constants in the general equation of the $N^{\text{th}}$ order (or class): and, what is of much importance, we have the equation not only without the labour of elimination, but free from all superfluous terms and factors; while the most compendious expressions which modern geometry employs to represent curves and surfaces, would, if written out at length in terms of the co-ordinates included in the factors, be generally found to contain an enormously disproportionate amount of superfluous terms.

Generally, if $n$ be not greater than the number of constants in the general equation of the $\mathrm{N}^{\text{th}}$ degree, the expression

$$\Sigma \pm \overset{a\,b\,c}{\underset{0\,0\,0}{xyz}} \cdot \overset{a'b'c'}{\underset{1\,1\,1}{xyz}} \cdot \overset{a''b''c''}{\underset{2\,2\,2}{xyz}} \,\ldots\ldots\ldots\, \overset{a_{\prime}}{\underset{n-1}{x}}\ \overset{b_{\prime}}{\underset{n-1}{y}}\ \overset{c_{\prime}}{\underset{n-1}{z}} \cdot \overset{0}{\underset{n}{x}}\ \overset{0}{\underset{n}{y}}\ \overset{0}{\underset{n}{z}} = 0$$

is the equation to $a$ surface of the $\mathrm{N}^{\text{th}}$ degree passing through the $n$ points $x_1 y_1 z_1$, $x_2 y_2 z_2$, &c., where the indices $a\ b\ c$ are any positive numbers, zero included, different or alike, as are also $a'\ b'\ c'$, &c.; provided first, that at least one set of three has a sum $a + b + c = \mathrm{N}$; secondly, that no set (except the last) be all three zeros; thirdly, that no set has a sum greater than N; fourthly, that in no pair of sets $a'b'c'$, and $a_{\prime}b_{\prime}c_{\prime}$, we have at once $a' = a_{\prime}$, $b' = b_{\prime}$, and $c' = c_{\prime}$; and, lastly, that the indices are not reduced to $a\ a^1 \ldots$ alone —to $b\ b^1 \ldots$ alone—or to $c\ c^1 \ldots$ alone.

In the same manner, we can write out at once the equation to a locus of any degree for a geometry of four dimensions, the defining points being $(x_1\, y_1\, z_1\, w_1)$, $(x_2\, y_2\, z_2\, w_2)$, &c.; or with equal facility for a geometry of any number of dimensions.

Thus, for example, $\Sigma \pm x_0^2 \cdot y_1 \cdot 1_2 = 0$, is a parabola having for diameter the axis of $y$, and passing through the points 1 and 2; $\Sigma \pm x_0 y_0 \cdot 1_1 = 0$ is an hyperbola through 1, whose asymptotes are the axes; $\Sigma \pm x_0 y_0 \cdot x_1 \cdot y_2 \cdot 1_3$ is an hyperbola through 1, 2, and 3, whose asymptotes are parallel to the axes; $\Sigma \pm (x_0^2 \pm y_0^2) \cdot x_1 \cdot y_2 \cdot 1_3 = 0$ and $\Sigma \pm (x_0^2 \pm y_0^2) \cdot 1_1 = 0$ are conics referred to equal conjugate diameters which are parallel to the axes; these curves are either circles or equilateral hyperbolas, if the axes chosen are rectangular. The additional terms are in every case to be formed by permutation of the sub-indices

So far as I am aware, this is a view of equations to geometrical loci which has not been given before. I know that the shape which the results of elimination must assume, is no secret to analysts; but has this simple mode of stating

these geometrical results, so easy to remember, and so easy of demonstration, been previously laid down?

We return to our paradigm $\Sigma \pm x_0^2 . y_1^2 . z_2^2 . x_3 y_3 . y_4 z_4 . z_5 x_5 . x_6 y_7 . z_8 . 1_9 = 0$, considering it as a purely geometrical datum. It is necessary that we should both interpret and prove this proposition ($\Sigma = 0$), without borrowing any aid from arithmetic, and that we should show how a surface of the second order is given thereby, and can be therefrom constructed. The interpretation is more easy to be given than to be understood. Every one can conceive the reality denoted by $xy$, a parallelogram having a certain angle, or that represented by $x\ y\ z$, a prism whose edges have given inclinations. But what geometrical entity is $x\ y\ z\ w$, or $x\ x\ z\ y$? It is assuredly a volume or solid of four dimensions; for the product of four right lines can be only a figure of some kind, which does not straightway become an absurdity because the inhabitants of this planet find it difficult to imagine its existence. In like manner $x_0^2 . y_1^2 . z_2^2 . x_3 y_3 . y_4 z_4 z_5 x_5 . x_6 . y_7 . z_8$, the product of fifteen right lines, represents a volume of fifteen dimensions, and the proposition before us ($\Sigma = 0$) asserts that a given number of such volumes, constructed of course in space of fifteen dimensions, and having edges equal to certain lines given in common space, viz., the co-ordinates of our ten points, 1 2 3...9 0, have a sum equal to zero.

If the reader feel distressed with the effort to imagine such transcendental volumes in space of more than three dimensions, that is no affair of mine; my duty being, not to supply him with additional senses, but with sound arguments, of which he is competent to judge with even fewer than five. Had the reader been so unhappy as to enter this world deprived of the sense of touch, he would probably have been as much in the dark about the geometrical import of $x . y . z$, as he now is, being endowed with only five senses, concerning the real existence of these solids of fifteen

dimensions; yet he might not have been less qualified to judge of the logic of tri-dimensional geometry, nor less profited by the devices which it employs for the solution of problems in a plane. I beg the reader to believe, that to the mathematicians in the planet Mercury, the outward apperception of these entities is a very different affair from what he finds it now.

All that is incumbent on me is to show, that the proposition $\Sigma + x_0^2 \cdot y_1^2 \cdot z_2^2 \cdot x_3y_3 \cdot y_4z_4 \cdot z_5x_5 \cdot x_6 \cdot y_7 \cdot z_8 \cdot 1_9 = 0$ expresses the law of the solid locus of the second degree. I proceed to prove geometrically that every point $x_0y_0z_0$, which by its co-ordinates satisfies the proposition, lies in a continuous locus, such that no right line can meet it in one point only, or in more than two.

It is conceded readily, that in a product of lines, as in a product of numbers, the order of the factors is indifferent, so that

$$x_0^2 \cdot y_1^2 \cdot z_2^2 \cdot x_3y_3 \cdot y_4z_4 \cdot z_5x_5 \cdot x_6 \cdot y_7 \cdot z_8 = x_0^2 \cdot x_3 \cdot x_5 \cdot x_6 \cdot y_1z_2 \cdot y_1z_2 \cdot y_4z_4 \cdot y_3z_5 \cdot y_7z_8 = \mathrm{A}.$$

Let O be the origin, and taking any three points on the positive axes, $x$, $y$, $z$, let

$$Ox = \xi, \quad Oy = \eta, \quad Oz = \zeta \, .$$

The Cartesian co-ordinates being drawn, we have in the plane of $yz$ the parallelogram $y_7z_8$, having an angle at O. Join the extremity of $y_7$ to $z$, the extremity of $\zeta$; and from that of $z_8$ draw a parallel to this joining line, cutting the axis of $y$ at a distance $e$ from O. We have plainly $\zeta : z_8 = y_7 : e$, or the parallelograms $\zeta e$ and $y_7z_8$ are equal, $e$ being a length cut off from the origin on the axis of $y$, and positive or negative according as $y_7$ and $z_8$ have like or unlike signs.

By drawing four more pairs of parallel lines, after the manner of the pair just drawn, we can reduce A to the form $\mathrm{A} = x_0^2 \cdot y_1^2 \cdot z_2^2 \cdot x_3y_3 \cdot y_4z_4 \cdot z_5x_5 \cdot x_6 \cdot y_7 \cdot z_8 = x_0^2 \cdot x_3 \cdot x_5 \cdot x_6 \cdot \zeta e \cdot \zeta e_1, \cdot \zeta e \; \cdot \zeta e_3 \cdot \zeta e_4$, $e\ e_1\ e_2\ e_3\ e_4$ being lengths cut off from O on the axis of $y$, and positive or negative as the case may be, by the drawing of the described five pairs of parallels.

What we have done in the plane of $y\,z$ by the aid of our $z$-unit $\zeta$, we can imitate by drawing pairs of parallels, in the plane of $x\,y$, by the aid either of $\eta$ or $\xi$; for $x_0\,e_1$, $x_3\,e_2$, &c., are given rhomboids in that plane, having each an angle at O. Three pairs of parallels will effect the transformation,

$A = \zeta^5 x_0^2 \cdot e \cdot e_1 \cdot x_3 e_2 \cdot x_5 e_3 \,.\, x_6 e_4 = \zeta^5 \eta^3 \cdot x_0^2 \cdot e e_1 \,.\, \mathrm{x}_1 \mathrm{x}_2 \mathrm{x}_3$, in which $x_3 e_2 = \eta \mathrm{x}_1$, $x_5 e_3 = \eta \mathrm{x}_2$, $x_6 e_4 = \eta \mathrm{x}_3$; and a fourth pair gives $A = \zeta^5 \eta^4 \cdot x_0^2 e \,.\, \mathrm{x}_4\, \mathrm{x}_2\, \mathrm{x}_3$; which, by three pairs more, and the equations $e\mathrm{x}_4 = \xi e_5$, $e_5 \mathrm{x}_2 = \xi e_6$, $e_6 \mathrm{x}_3 = \xi \mathrm{Y}$, becomes $A = \zeta^5 \eta^4 \xi e_5 \mathrm{x}_2 \mathrm{x}_3 x_0^2 = \zeta^5 \eta^4 \xi^2 e_6 \mathrm{x}_3 x_0^2 = \zeta^5 \eta^4 \xi^3 \mathrm{Y} x_0^2$;

$\mathrm{x}_1 \mathrm{x}_2 \mathrm{x}_3$ being lengths cut off from O on the axis of $x$, and $e_5\; e_6$ and Y lengths on that of $y$, where Y is positive or negative with A. Thus, by drawing twelve pairs of parallels, we effect the reduction

$$A = x_0^2 \,.\, \underset{1}{y^2} \,.\, \underset{2}{z^2} \,.\, \underset{3\,3}{xy} \,.\, \underset{4\,4}{yz} \,.\, \underset{5\,5}{zx} \,.\, \underset{6}{x} \,.\, \underset{7}{y} \,.\, \underset{8}{z} \,.\, \underset{9}{1} = \zeta^5 \eta^4 \xi^3 \mathrm{Y} \,.\, x_0^2.$$

The term, $B = -\, x_0^2 \,.\, y_1^2 \,.\, z_2^2 \cdot x_3 y_3 \cdot y_4 z_4 \cdot z_5 x_5 \cdot x_6 \cdot y_8 \cdot z_7 \,.\, 1_9 = \zeta^5 \eta^4 \xi^3 \mathrm{Y}_{,}\, x_0^2$, is reduced in the same manner, $\mathrm{Y}_{,}$ being a length from O on the axis of $y$, and positive or negative with B, the sign of which depends on the co-ordinates by which that solid is determined.

Let B be supposed positive; then the addition

$$A + B = \zeta^5 \eta^4 \xi^3 \,(\mathrm{Y} + \mathrm{Y}_{,})\, x_0^2,$$

is to be performed. If $c$ and $c_{,}$ be the extremities of Y and $\mathrm{Y}_1$ remote from O, and $bc$ the line intercepted between the axis that cuts off $\mathrm{Y} = Oc$; draw $cb_{,}$ parallel to the axis of $x$ to $b_{,}$ in $b\,b_{,}$ parallel to that of $y$; join $bc_{,}$, and parallel to this draw $b_{,}c_{,,}$ to $c_{,,}$ in the axis of $y$; the length $Oc_{,,} = \mathrm{Y} + \mathrm{Y}_1$. This requires the three lines $bc_{,}$ $cb_{,}$ $b_{,}c_{,,}$, $bc$ and $bb_{,}$ having been drawn before. The subtraction of $\mathrm{Y}_{,}$ from Y might have been performed with equal facility, giving $\mathrm{Y}\text{-}\mathrm{Y}_{,}$, a length cut off from O.

The sum of all the terms of the paradigm which contain $x_0^2$, can thus be reduced to a single term $\zeta^5 \xi^3 \eta^4\ \mathrm{K}\, x_0^2$, in which

K is a length found by the ruler only on the axis of $y$, and positive or negative with the sum of the solids of fifteen dimensions, which is thus proved equivalent to one single solid of the same dimensions; an equivalence rigorously geometrical, and of the truth of which the reader can judge for himself, although he has my word only for the existence of such solids.

The term

$$C = x^2_6 . y^2_1 . z^2_2 . x_3y_3 . y_4z_4 . z_5x_5 . x_0 . y_7 . z_8 . 1_9 = x_0 . x_6 . x_6 . x_3 . x_5 . y_1z_2 . y_1z_2 . y_4z_4 . y_3z_5 . y_7z_8$$

can be reduced by drawing thirteen pairs of parallels, to the equivalent $C = \zeta^5\xi^4\eta^4\, x_0\, Y$, which is a solid equal in geometrical value to C. The whole of the terms containing $x_0$ can thus be collected into one term, $\zeta^5\xi^4\eta^4\, K_{,}\, x_0$.

Let us suppose that we are in quest of all the points $(x_0 y_0 z_0)$ in any line, passing through one of the given nine, whose co-ordinates $x_0$ $y_0$ and $z_0$ satisfy the geometrical condition,

$$\Sigma \pm x^2_0 . y^2_1 . z^2_2 . x_3y_3 . y_4z_4 . z_5x_5 . x_6 . y_7 . z_8 . 1_9 = 0.$$

Let the line pass through the point 9; let this be taken for the origin, and the line for the axis of $x$. We shall have $x_9 = y_9 = z_9 = 0 = y_0 = z_0$ in our paradigm, which will thus contain no terms free from $x_0$. The proposition before us is then equivalent to

$$x_0\zeta^5\xi^3\eta^4\{Kx_0 - \xi K_1\} = 0,$$

which is satisfied by all the points which we are seeking on the axis of $x$, K and $K_1$ being lengths on that of $y$ from O, found with the ruler only, and $\xi$ being the $x$-unit, arbitrarily assumed on the axis of $x$. There is plainly only one such point, which is found by joining the extremities of $\xi$ and K, and then from the extremity of $K_{,}$ drawing a parallel to the joining line: the parallel cuts the axis of $x$ in the point required. If $K_{,} = o$, the axis of $x$ is a tangent of the surface at the origin. We have thus proved that our paradigm represents a surface, for it is the locus of a point which can be assigned more than once upon any line

through one of the point's positions; it is also a surface of the second order, since no line can meet it once only, or more than twice. That this locus passes through all the nine given points, is proved by the same a'gument when the paradigm is treated as a purely geometrical datum, as that which has been adduced when we considered it as an analytical datum. The expression, namely,

$$\Sigma \pm x_0^2 \cdot y_1^2 \cdot z_2^2 \cdot x_3 y_3 \cdot y_4 z_4 \cdot z_5 x_5 \cdot x_6 \cdot y_7 \cdot z_8 \cdot 1_9$$

is zero, whenever the tenth point $o$ coincides with any of the given nine; for the solids that form it destroy each other in pairs.

If $x_0\ y_0\ z_0$ be any tenth point on the surface, the term A can be reduced by the drawing of fourteen pairs of parallels to the form $A = \zeta^5 \eta^5 \xi^4\, x$; and every term in the paradigm can be by the same labour reduced to the same form; the addition of the lines $x\ x_1\ x_2\ x_3$, &c., thus found on the axis of $x$ must give the result.

$$\zeta^5 \eta^5 \xi^4 (x + x_1 + x_2 + x_3 + \dots \qquad) = 0,$$

which is the condition, both necessary and sufficient, in order that the ten points, 1 2 3...9 0, should lie in a surface of the second order. The addition of these lines $x\ x_1$, &c., is effected by drawing certain lines, in sets of three each, the last of which lines will pass through the origin.

*If, then, any eleven points in space be taken, the condition that any ten shall lie on a surface of the second order is, that a line, found by the ruler only, shall pass through the eleventh.*

This is a purely geometrical solution, by the aid of the ruler only, of the Brussels Academy's prize question. The found line is one of an infinite number forming a pencil through the eleventh point—the particular line depending on the axes drawn through that point.

The paradigm of the general surface of the $n^{\text{th}}$ order can always be transformed by drawing of given lines with the ruler—the axis of $x$ being drawn through the point $(x_0, o, o)$,

whose relation to the surface we are examining, to the form

$$H\{Yx^n + \xi Y_1 x^{n-1} + \xi^2 Y_2 x^{n-2} + \ldots\ldots + \xi^{n-1} Y_{n-1} x + \xi^n Y_n\} = 0,$$

where H is a factor that may be disregarded, and $Y\ Y_1\ Y_2 \ldots$ are lengths from $o$ on the axis of $y$, and $\xi$ is an arbitrary length on that of $x$. If now $n$-1 of the points that define the surface are on the axis of $x$, the $n^{th}$ point in which that axis meets the surface, is found by drawing a small number of additional parallels.

The expression before us—which, like the paradigm of which it is the reduction, is a purely geometrical datum and proposition—is an equation whose co-efficients and roots are not numbers but lines. Since the addition and multiplication of lines are subject to the same laws of aggregation and commutation with those of numbers, there is no reason in the world why the doctrine of equations, to a certain point at least, should not be property common both to arithmetic and geometry; so far, namely, as the symbols in the theory of equations retain their perfect generality. By this theory, we know that S, the sum of the roots of the above equation, is given by the proportion

$$- S : \xi = Y_1 : Y,$$

and — S is found by the drawing of two parallels. It has been already shown that the addition and subtraction of lines from lines on the axis of $x$ can be effected by the ruler; hence the sought point is obtained by subtracting from + S, thus constructed, the known sum of the other $n$-1 roots of the equation.

All that is proved above concerning loci of the $N^{th}$ order holds for constructions in loci of the $N^{th}$ class; if the co-ordinates, $x\ y\ z$, &c., determine not points, but lines or planes. If the axes of the line or plane co-ordinates are parallel axes, as they may be if we so choose, I think it will be found that all the constructions in question will be effected

by the drawing of given converging lines; so that a finite ruler will in general solve the problems.

I readily allow that these linear constructions, although they will, as I flatter myself, be found rigorously geometrical, are far from being reduced to their most simple form; and I could state, if space were allowed me, methods of abridging the operations indicated. If the locus under consideration be represented not by Cartesian co-ordinates, but by a system of terms, each being a product of linear functions of $x$ $y$ and $z$, which represent distances from given lines or planes measured in a determined direction, a slight modification of the above method of using the ruler, will often bring out the required result with comparatively little labour. I shall content myself with one example of a more compendious method, forming the solution of a problem of remarkable interest; to find the ninth intersection of two curves of the third order through eight given points.

The principal difficulty in the solution of the ninth point problem, lies in the finding linearly a fifth known point on each of the two conics through the points 12349 and 82349, (*vide* page 83 of the 6th vol. of the Cambridge and Dublin *Mathematical Journal*),

$$[123458]\,[123467]\,(67)\,(58) - [123457]\,[123468]\,(68)\,(57) = 0 = (12349);$$

$$[123458]\,[823467]\,(67)\,(51) - [823457]\,[123468]\,(61)\,(57) = 0 = (82349);$$

where [123458] is the integral function of the co-ordinates of the six points 123458, which vanishes if they are on a conic; the *aconic function* [123458], as it has been denominated by Sir W. R. Hamilton, and (67) = o, denotes the integral equation of the line through 6 and 7.

A fifth point on the conic (12349) is the intersection of the two lines

$$[123458]\,(58) - [123457]\,(57) = o = u$$

$$[123468]\,(68) - [123467]\,(67) = o = v.$$

I shall proceed to draw these lines by the linear construction of the aconic functions in their equations. Putting

$$\frac{(y_1 - y_2)(y_4 x_5 - y_5 x_4) - (y_4 - y_5)(y_1 x_2 - y_2 x_1)}{\overline{y_1 - y_2} \cdot \overline{x_4 - x_5} - \overline{y_4 - y_5}\, \overline{x_1 - x_2}} = y^{12}_{45}$$

and $x^{12}_{45}$ for what this becomes when $y_1$ is exchanged for $x_1$, &c.; the area of the *Pascalian triangle*, having its angles at the intersections of the opposite sides of the hexagon, 123458 is,

$$\triangle = y_{\substack{12\\45}} \cdot (x_{\substack{23\\58}} - x_{\substack{34\\81}}) + y_{\substack{23\\58}} (x_{\substack{34\\81}} - x_{\substack{12\\45}}) + y_{\substack{34\\81}} (x_{\substack{12\\45}} - x_{\substack{23\\58}}) = \mathrm{C} : \mathrm{D},$$

C being the aconic function [123458] and D being the factor

$$(\overline{y_1 - y_2} \cdot \overline{x_4 - x_5} - \overline{y_4 - y_5} \cdot \overline{x_1 - x_2})(\overline{y_2 - y_3} \cdot \overline{x_5 - x_8} - \overline{y_5 - y_8} \cdot \overline{x_2 - x_3})(\overline{y_3 - y_4} \cdot \overline{x_8 - x_1} - \overline{y_8 - y_1} \cdot \overline{x_3 - x_4})$$

which vanishes whenever $\triangle$ becomes infinite, *i. e.*, when any pair of opposite sides of the hexagon 123458 are parallel. That C $= o$ is the equation to the conic when this hexagon lies in one, is evident from the consideration that it is of the second degree in $(x_8 y_8)$, and that it vanishes if for $(x_8 y_8)$ you put any of the other five, $(x_1 y_1)$ $(x_2 y_2)$ $(x_3 y_3)$ . . . If $(x_8 y_8)$ is $(x_1 y_1)$ or $(x_5 y_5)$, this is instantly seen; and if $(x_8 y_8)$ is $(x_2 y_2)$, the triangle $\triangle$ has all its angles in (12); if $(x_8 y_8)$ is $(x_4 y_4)$, it has all its angles in (54); and if $(x_8 y_8)$ is $(x_3 y_3)$, two of its angles coincide with the point 3, in none of which three cases does D become zero.

Wherefore, if the points (12·45), (23·58), and (34·81), be $a$, $c$, $e$,

$$[123458] = \left[y_a \cdot \overline{x_c - x_e} + y_c \cdot \overline{x_e - x_a} + y_e \cdot \overline{x_a - x_c}\right] \mathrm{D},$$

$$[123457] = \left[y_a \cdot \overline{x_{c_1} - x_{e_1}} + y_{c_1} \cdot \overline{x_{e_1} - x_a} + y_{e_1} \cdot \overline{x_a - x_{c_1}}\right] \mathrm{D}_1,$$

where $c_1$ $e_1$ $D_1$ differ from $c$ $e$ $D$ only by the exchange of 8 for 7. Neglecting the common factor of D and $\mathrm{D}_1$, which is free from 8 and 7, we have to draw the line

$$\left\{\begin{array}{l} (y_a . \overline{x_c - x_e} + y_c . \overline{x_e - x_a} + y_e . \overline{x_a - x_c})(\overline{y_2 - y_3} . \overline{x_5 - x_8} - \overline{y_5 - y_8} . \overline{x_2 - x_3})(\overline{y_3 - y_4} . \overline{x_8 - x_1} - \overline{y_8 - y_1}\, \overline{x_3 - x_4}) . (58) \\ -(y_a . \overline{x_{c_1} - x_{e_1}} + y_{e_1} . \overline{x_{e_1} - x_a} + y_{e_1} . \overline{x_a - x_{c_1}})(\overline{y_2 - y_3} . \overline{x_5 - x_7} - \overline{y_5 - y_7} . \overline{x_2 - x_3})(\overline{y_3 - y_4} . \overline{x_7 - x_1} - \overline{y_7 - y_1}\, \overline{x_3 - x_4}) . (57) \end{array}\right\} = o \quad u,$$

or, taking for our axes of $x$ and $y$, the lines (34) and (32), putting thus $x_3 = y_3 = x_2 = y_4 = x_e = x_1^e = y_e = y_1^e = o$,

$$(y_c \cdot \overline{x_e - x_a} - y_a x_e)(y_2 \cdot \overline{x_5 - x_8})(\overline{y_8 - y_1} \cdot x_4)(58) - (y_{c'} \cdot \overline{x_{e'} - x_a} - y_a x_{e'})(y_2 \cdot \overline{x_5 - x_7})(\overline{y_7 - y_1} \cdot x_4)(57) = u = o,$$

is the line which is to be drawn; where

$a$ is the point 12 . 45, $c$ is the point 23 . 58, $e$ is 34 . 81
$c_1$ 23 . 57, $e_1$ is 34 . 71

Let the co-ordinates of the eight points be drawn, which amount to twelve lines besides the axes, and on these let *any* two positive lengths from $O$ the origin, as $y_2 = k$, $x_4 = m$, be assumed.

Let that diagonal of any parallelogram made by the co-ordinates produced, which produced cuts from the axes segments of like sign, be called positive, and negative when those signs are unlike.

The rhomboid $k \cdot \overline{x_e - x_a}$ is a determined portion of the figure; and has a side $x_e - x_a$ in the axis of $x$. Draw that diagonal $d$ of it which has the sign of $x_e$-$x_a$, and parallel to $d$ from $c$, draw $c$ A to A in the axis of $x$; then is $y_c . (x_e - x_a) = k$. OA. Draw the ordinate $\mathrm{AA}_1$, meeting $x_a$ in $\mathrm{A}_1$; draw the diagonal $d_1$ of the rhomboid $k\, x_e$ which differs in sign from $x_e$, and parallel to $d_1$ draw $\mathrm{A}_1\mathrm{X}$ to X in the axis of $x$: then is $k$.AX $= - y_a\, x_e$ and $k$.OX $= y_c \cdot (x_e$-$x_a) - y_a\, x_e$. Draw next $d_2$ the diagonal of $k \cdot (x_5 - x_8)$ like-signed with $\overline{x_5 - x_8}$, and parallel to $d_2$ draw from 2 to $\mathrm{X}_1$ in the axis of $x$ the line 2 $\mathrm{X}_1$ : $y_2 . (x_5 - x_8) = \mathrm{K.OX}_1$. Parallel to 24 draw a line $\mathrm{O}_1$ B, meeting on the axis of $y$ at $\mathrm{O}_1$ one of the abscissæ $x_8$ and $x_1$ so as to cut the other in B on the positive or negative side of that axis, according as $\overline{y_8 - y_1}$ is positive or negative. The ordinate $BX_2$ will cut off $OX_2$ such that $K \cdot OX_2 = \overline{y_8 - y_1} \cdot x_4$. Thus by drawing the ten lines $d$, $c$ $A$, $AA_1$, $d_1$, $A_1X$, $d_2$, $2X_1$, 24, $O_1B$, $BX_2$, we have effected the reduction

$$(y_c \cdot \overline{x_e - x_a} - y_a x_e)(y_2 \cdot \overline{x_5 - x_8})\, \overline{y_8 - y_1} \cdot x_4 = k^3\, OX\, OX_1\, OX_2.$$

Join now 2 $X_2$, and draw the lines $Xk_{,}$, $k_{,}X'$, $X'k_{,,}$, $k_{,,}X''$; $Xk_{,}$ and $X'k'$ both parallel to 24, and $k_{,}X'$ and $k_{,,}X''$ in order

parallel to $2X_{,}$ and $2X_2$; $k_{,}$ and $k_{,,}$ being in the axis of $y$, and $X'$ and $X''$ in that of $x$. Then since $m : OX = OX_{,} : OX'$, and $m : OX' = OX_2 : OX''$, $OX \,.\, OX_{,} \,.\, OX_2 = m^2\, OX''$, and $(y_{e} \,.\, \overline{x_{e}-x_{a}} - y_{a}\, x_{e})(y_2 \,.\, \overline{x_5 - x_8})\, \overline{y_8 - y_1} \,.\, x_4 = k^3\, m^2\, OX''$; a transformation effected by 15 applications of the ruler, when the Cartesian co-ordinates, and the four lines determining the points $a$ $e$ $c$, have once been drawn.

Let now 57 and 71 be drawn to $c_{,}$ and $e_{,}$: let the diagonals of $k\, \overline{x_{e,} - x_{a}}$, $k\, \overline{x_5 - x_7}$, like-signed with $\overline{x_{e,} - x_{a}}$ and $\overline{x_5 - x_7}$, and that of $k\, x_{a_{e,}}$, of the sign contrary to that of $x_{a_{e,}}$, be drawn. Four parallels and two ordinates, after the manner of $c$ A, $A_1X$, $2X_1$, $O_1B$, $AA_1$ $BX_2$ above, will suffice to determine the points x $x_1$ $x_2$ such that

$$(y_{e,} \,.\, \overline{x_{e,} - x_{a}} - y x_{a e,})\,(y_2 \,.\, \overline{x_5 - x_7})\,(\overline{y_7 - y_1} \,.\, x_4) = k^3 .\ \mathrm{Ox}.\ \mathrm{Ox}_1\ \mathrm{Ox}_2\,;$$

and by drawing five lines after the manner of $2X_2$, $Xk_1$, $k_1X^1$, $X'k_{,,}$, $k_{,,}X''$ above, we shall obtain the point x$''$ on the axis of $x$ such that

$$(y_{e_{,}} \,.\, \overline{x_{e_{,}} - x_{a}} - y x_{a e_{,}})\,(y_2 \,.\, \overline{x_5 - x_7})\,(\overline{y_7 - y_1} \,.\, x_4) = k^3 m^2 \mathrm{Ox}''.$$

consequently the line to be drawn is

$$OX.''\,(58) - \mathrm{Ox}''\,57 = o = u.$$

Join 78; let 8x$''$ meet $7X''$ in $p$; let $Op$ meet $x_7$ in $q$; let $qr$ parallel to 8x$''$ meet 78 in $r$; then $5r$ is the line required. This is constructed at the expense of 40 applications of the ruler, besides the drawing of the Cartesian co-ordinates of the eight points.

It is necessary in the next place to find the line

$$(y_d \,.\, \overline{x_e - x_b} - y x_{be})\,(y_2 \,.\, \overline{x_6 - x_8})\,(\overline{y_8 - y_1} \,.\, x_4)\,(68) - (y_{d_1} \,.\, \overline{x_{e_1} - x_{b_1}} - y x_{b e_{11}})\,(y_2 \,.\, \overline{x_6 - x_7})\,(\overline{y_7 - y} x_4)\,(67) = o = v$$

where $b = 12\,.\,46$; $d = 23.68$, $e = 34.81$
$d_{,} = 23.67$, $e_1 = 34.71$;

By drawing, in addition to the lines, 46, 68, 67, the diagonals of $k\, \overline{x_e - x_b}$, $k\, \overline{x_6 - x_8}$, with proper signs, then three pa-

rallels and an ordinate, after the manner of $c\,A$, $A_1X$, $2X_1$, $AA_1$, we obtain two points $X_3X_4$ such that

$$(y_d\,.\,\overline{x_e - x_b} - y_bx_e)\,(y_2\,.\,\overline{x_6 - x_8})\,(\overline{y_8 - y_1}\,.\,x_4) = k^3\,OX_4\,OX_3\,OX_2.$$

Four lines more, setting out from $X_4$, after the manner of the four $Xk_{,}$, $k_{,}X'$, $X'k_{,,}$, $k_{,,}X''$, will determine the point $S$ on the axis of $x$ such that $OX_4\ OX_3\ OX_2 = m^2\ OS$, or

$$(y_d\,.\,\overline{x_e - x_b} - y_bx_e)\,(y_2\,.\,\overline{x_6 - x_8})\,(\overline{y_8 - y_1}\,.\,x_4 = k^3m^2 O\,S.$$

Next, the co-efficient of (67) in $v = o$ can be reduced by drawing the diagonals of $k\,(x_1^e - x_1^b)$, $k\,(x_6 - x_7)$, and then as above three parallels and an ordinate, to $k^3\,Ox_4\,Ox_3\,Ox_2$, which four lines now will transform to $k^3\,m^2\,Os$, $s$ being a point found on the axis of $x$. Thus twenty-three new lines are expended in transforming the problem to the shape

$$OS\,.\,(68) - Os\,(67) = o = v,$$

and $r_{,}$ in the sought line $6r_{,}$ is obtained by drawing other four. Draw now $5r$ and $6r_{,}$ to meet in $P$; and we have found a fifth point of the conic ( 12349) by 67 applications of the ruler, if the Cartesian co-ordinates are drawn beforehand.

We have yet to construct the pair of lines

$$[234581]\,(51) - [234587]\,(57) = o = u_1\ ,$$
$$[234681]\,(61) - [234687]\,(67) = o = v_1\ .$$

Now $[234587] = [y_c(x_{e_{,,}} - x_{a_{,}}) + y_{e_{,,}}(x_{a_{,}} - x_c) + y_{a_{,}}(x_c - x_{e_{,,}})]$ $.D^1$, $a_1$ being (72·45), $e_{,,}$ being (34·87), and $D^1$ being the quantity

$$(\overline{y_2 - y_3}\,.\,\overline{x_5 - x_8} - \overline{y_5 - y_8}\,.\,\overline{x_2 - x_3})\,.\,(\overline{y_3 - y_4}\,\overline{x_8 - x_7} - \overline{y_8 - y_7}\,\overline{x_3 - x_4})\,.\,(\overline{y_4 - y_5}\,.\,\overline{x_7 - x_2} - \overline{y_7 - y_2}\,\overline{x_4 - x_5}),$$

Omitting from D and $D^1$ the first of these three factors, the line $u_1 = 0$ is

$$y_c\,.\,\overline{x_e - x_a} - y_ax_e)\,(\overline{y_1 - y_2}\,.\,\overline{x_4 - x_5} + y_5x_1)\,(\overline{y_8 - y_1}\,.\,x_4)\,(51) - (y_o\,.\,\overline{x_{e_{,,}} - x_{a_{,}}} - y_{a_{,}}x_{e_{,,}})\,(\overline{y_7 - y_2}\,.\,\overline{x_4 - x_5} + y_5x_7)\,(\overline{y_8 - y_7}\,.\,x_4)\,(57) = o$$

Draw 72 and 87, to find $a_{,}$ and $e_{,,}$ on 45 and 34. Draw the diagonals of $k\,(x_4 - x_5)$, $kx_1$, $k\,\overline{x_{e_{,,}} - x_{a_{,}}}$, $kx_7$, having the signs of these areas, and that of $kx_{e_{,,}}$ having its sign opposite to that of $x_{e_{,,}}$. A parallel to the first of these five diagonals meeting the axis of $y$ on *one* of $x_1$, $x_2$, as the case requires, will intercept

at F on *the other* an absciss having the sign of $y_1 - y_2$. Draw the ordinate $FF^1$ meeting $x_5$ in $F''$, and next $F''X_5$ parallel to the second diagonal to $X_5$ in the axis of $x$.

then is $k\,.\,O\underset{5}{X} = k.\,(OF^{11} + F^{11}\underset{5}{X}) = \overline{\underset{1}{y} - \underset{2}{y}}\,.\,\overline{\underset{4}{x} - \underset{5}{x}} + \overline{\underset{5}{y}}\,\underset{1}{x}$,

and $(\underset{a}{y}\,.\,\overline{\underset{c}{x} - \underset{e}{x}} - \underset{a}{y}\,\underset{e}{x})\,(\overline{\underset{1}{y} - \underset{2}{y}}\,\overline{\underset{4}{x} - \underset{5}{x}} + \underset{5}{y}\,\underset{1}{x})\,(\overline{\underset{8}{y} - \underset{1}{y}}\,.\,\underset{4}{x}) = k^3\ OX\,.\,O\underset{5}{X}\,O\underset{2}{X}$

Draw now from $c$ a parallel to the third diagonal, meeting the axis of $x$ in G, and from the intersection $G_1$ of the ordinate $GG_1$, with $x_{a_{,}}$, draw $G_1x'$ to $x^1$ in the axis of $x$, parallel to the diagonal of $kx_{e_{,,}}$, the last of the five. We have thus $y_c\,.\,\overline{x_{e_{,,}} - x_{a_{,}}} - y_{a_{,}}x_{e_{,,}} = k\,.(O\,G_1 + G_1x^1) = k.Ox^1$. Next let a parallel to the first diagonal meet either $x_7$ or $x_2$ on the axis of $y$, intercepting at H, on the other, an absciss having the sign of $y_7 - y_2$; and from $H^1$, the intersection of the ordinate $HH^1$ with $x_5$ draw a parallel to the fourth diagonal, to $x_1'$ in the axis of $x$. Then is $k\,.\,Ox_1' = k\,.\,(OH + H^1x_1') = \overline{y_7 - y_2}\,.\,\overline{x_4 - x_5} + y_5\,x_7$; and by drawing a parallel to 24, and an ordinate meeting the axis of $x$ in $x'_2$, we obtain

$$(y_c\,.\,\overline{x_{e_{,}} - x_a} - y_{a_{,}}x_{e_{,}})\,(\overline{y_7 - y_2}\,\overline{x_4 - x_5})\,(\overline{y_7 - y_1}\,.\,x_4) = k^3\,.\,Ox'\,.\,Ox'_1\,.\,Ox'_2$$

By drawing, in addition to $X\,k^1$ before drawn, the four $2\,X_5$, $k^1X_6$, $X_6k_1^1$, $k_1^1X_7$, $X_6k_1^1$, parallel to 24 like $Xk^1$, $k^1X_6$ to $2\,X_5$, and $k_1^1X_7$ to $2\,X_2$, drawn before, we effect the reduction,

$$k^3.\ OX\,.\ OX_5\ OX_2 = k^3\,m^2\ OX_7$$

Drawing next $2\,x_1^1$ and $2\,x_2^1$, and a broken line of four strokes, beginning at $x^1$ and ending at $x_5$, we obtain $k^3.\ Ox^1.\ Ox_1^1.\ O\,x_2^1 = k^3\,m^2.\ O\,x_5$; and the line to be drawn is now

$$OX_7\,.\,51 - Ox_5\ 57 = 0 = u_1\quad .$$

Join 17; let $1\,x_5$ meet $7\,X_7$ in $p^1$; let $Op^1$ meet $x_7$ in $q^1$; let $q^1r^1$, parallel to $1\,x_5$, meet 17 in $r^1$; $5\,r^1$ is the line required. The point $r^1$ is found by drawing 32 lines additional to the 67 already drawn before.

The line $v^1$ is

$$(\underset{c_{,,}}{y}\,.\,\overline{\underset{e}{x} - \underset{a}{x}} - \underset{a}{y}\,.\,\underset{e}{x})\,(\overline{\underset{1}{y} - \underset{2}{y}}\,.\,\overline{\underset{4}{x} - \underset{6}{x}} + \underset{6}{y}\underset{1}{x})\,(\overline{\underset{8}{y} - \underset{1}{y}}\,.\,\underset{4}{x})(61) - (\underset{c_{,,}}{y}\,.\,\overline{\underset{e_{,}}{x} - \underset{a_{,}}{x}} - \underset{a_{,}}{y}\underset{e_{,}}{x})\,(\overline{\underset{7}{y} - \underset{2}{y}}\,.\,\overline{\underset{4}{x} - \underset{6}{x}} + \underset{6}{y}\underset{7}{x})\,(\overline{\underset{8}{y} - \underset{7}{y}}\,\underset{4}{x})\,(67) = o$$

The drawing of 14 additional lines will reduce this to

$$k^3.\ OX_8.\ OX_9.\ OX_2\ (61) = k^3\ Ox_6\ Ox_7\ Ox_8\ (67)\ ;$$

twelve more lines give us the reduction,

$$k^3 \cdot OX_8 \cdot OX_9 \cdot OX_2 = k^3m^2\ OT$$
$$k^3 \cdot Ox_6 \cdot Ox_7 \cdot Ox_8 = k^3m^2\ Ot.$$

and the line

$$OT \cdot (61) - Ot\ (67) = o = v_1$$

is given by drawing other four lines.

We have thus found a fifth point of the conic (82349) by drawing 32 + 30 lines additional to the 67 lines expended in finding a fifth point on (12349): in all, 129 lines.

We can now proceed, after the elegant method of Mr. Weddle, to find the point 9 by five applications of Pascal's theorem, so that the mystic enneagram is completed by at most 150 applications of the ruler, after the drawing of the Cartesian co-ordinates; a process which will be esteemed simplicity itself by those who have attempted to express $x_9$ and $y_9$ in terms of $x_1 y_1$, &c., or even in terms of their numerical values.

NOTE.—Since this paper was read, there has appeared in the *Cambridge and Dublin Mathematical Journal* of this year, 1851, a solution of this problem of the ninth point, with the ruler only, by the Rev. A. S. Hart, F.T.C.D., which may be pronounced perfect, and which for elegance and simplicity leaves nothing to be desired.

## XX.—*On the Analysis of Gaseous Mixtures.* By JOHN LEIGH, ESQ., M.R.C.S., F.C.S.

Read January 7, 1851.

THE chief object of this paper is an examination of the methods hitherto employed for determining the constituents of coal gas. Of late, considerable attention has been given by chemists to the products of the distillation of coal, as well as to those of its combustion, and of its spontaneous decomposition; and it is likely that their researches will erelong come to have a high value, and will throw much light on the great process of gas manufacture, on the economical employment of fuel as an agent in the production of steam, and in the great smelting operations of this country; and also in explanation of the production of the fire-damp, whose disastrous explosions so often occupy public attention.

I shall confine myself to an examination of the methods employed, analytical or otherwise, for determining the composition or the illuminating power of the gases resulting from the distillation of coal. There are few gaseous mixtures that have offered more difficulties to the chemist than those which make up the composition of coal gas; and as the time has now arrived, when a correct mode has become eminently desirable of ascertaining the proportions and constitution of the light-giving ingredients contained in the mixture, so that it may serve as a measure of the relative illuminating power of the gas, I will give a description of the method which I have been in the habit, for several years past, of employing in the examination of the gas produced at the Manchester gas-works.

A brief statement of the constituents of coal gas, as determined by numerous analyses, without reference to their relative proportions, by affording an idea of the objects of the analytical enquiry, may best precede an enquiry into the nature and value of the methods employed.

The gases eliminated from coal or cannel consist essentially of olefiant gas, volatile and condensible hydro-carbons, light carburetted hydrogen, hydrogen, carbonic oxide, carbonic acid, sulphuretted hydrogen, sulphuret of carbon, cyanogen, ammonia, and aqueous vapour. Of these the carbonic acid, sulphuretted hydrogen, and cyanogen, are positively injurious to health. The two latter, and ammonia, are injurious to the apparatus employed for the distribution of the gas, and these, with the sulphuret of carbon, to the furniture in rooms in which the gas is burnt. The nitrogen and oxygen generally found in gas, proceed from atmospheric air, which gains access by leakage of apparatus, and by opening the retorts when charging with fresh coal. In the process of purification now generally adopted at all well-regulated gas-works, the carbonic acid, sulphuretted hydrogen, cyanogen, and ammonia, are entirely removed; so that there remain olefiant gas, illuminating hydro-carbons, light carburetted hydrogen, hydrogen, carbonic oxide, and a minute portion of sulphuret of carbon, not recognizable by chemical tests in small quantities of the gas. Of these the hydrogen and carbonic oxide, though giving out much heat on combustion, yield scarcely any light, and burn with a very feeble blue flame; they only dilute the gas, adding nothing to its illuminating power. The light carburetted hydrogen burns with a yellow flame; the olefiant gas, and condensible hydro-carbons, with a very brilliant white flame, and give to the gas its chief illuminating power. The entire illuminating power of the gas, then, depends on the olefiant gas, hydro-carbons, and light carburetted hydrogen; the richness and value of a gas

may be determined by the proportions in which these exist in it, and a correct determination of their proportion and constitution will afford a correct and true measure of the quality and value of the gas, and a test of the excellence of the process by which the gas has been formed, as well as of the value of the coal, cannel, or other substance, used as a gas-producing material.

Gas-makers have been much in the habit of relying on the weight or specific gravity of gas as an indication of its quality. The heavier the gas, and, it is said, the better is its quality. This notion has arisen from observing, that olefiant gas is one of the heaviest of the constituents of coal gas. But the specific gravity is not to be depended upon as a test of the excellence of the gas, and in any case could only give a very crude idea of the general quality of it, without giving any knowledge of the nature and proportion of its constituents. The specific gravity of carbonic oxide is 967·8, and 100 cubic inches weigh 29·83 grains, at 60° Fahrenheit and 30 inches barometer. The specific gravity of olefiant gas is 985·2, and 100 cubic inches weigh 30·37 grains. The specific gravity of light carburetted hydrogen is 559·6, and 100 cubic inches weigh 17·25 grains. Now it is evident, that a gas containing much carbonic oxide, very little olefiant gas, and an inferior amount of light carburetted hydrogen, and consequently very poor in illuminating power, may weigh heavier, and seem better, than gas of far higher and better quality, so that the weight is only valuable as an adjunct to analysis.

The measurement of the light by the eye, whilst, like the above, affording no index of the constitution of the gas, is open to many irregularities and fallacies. There is no constant means of comparison, and after a few trials the eye fails to appreciate any but large differences. The next method of determining the illuminating power of gas, by estimating the amount of its constituents condensible by chlorine,

originated with the late Dr. Henry, and has of late been much recommended by Dr. Fyfe. The method consists in mixing chlorine with the gas, allowing the mixture to remain for some time in the dark, and then observing the diminution of bulk, and estimating half the diminution as olefiant gas. Chlorine has the property of combining with and condensing the olefiant gas and hydro-carbon contained in coal gas, whilst in the dark it exercises no action on the light carburetted hydrogen and other constituents. This is an attempt to estimate the value of a gas by the mere amount of olefiant gas that it contains; but the method of accomplishing this is liable to constant errors. Formerly the whole of the diminution of volume was observed, and one-half of the entire amount estimated as olefiant gas. It was overlooked that a portion of the chlorine itself underwent absorption, and increased the apparent diminution, thus giving too large a volume of olefiant gas. Dr. Fyfe detected this, and proposed, therefore, to observe first, how much chlorine was absorbed alone by the confining water, and then to deduct this amount from the total absorption, dividing the remainder as before. But this method is extremely fallacious, and, though still practised and strongly recommended by Dr. Fyfe, is utterly untrustworthy. Dr. Fyfe estimates the absorption of chlorine alone at 1 or 2 per cent. within the time employed for examination. I have found it to vary from 2 to 6 per cent. The rate of absorption varies with the diameter of the tube employed. But what is of more consequence, and completely vitiates Dr. Fyfe's results, or any results obtained by this process, is the fact, that the rate of absorption of chlorine varies with the dilution of the latter by any other gas. The rate of absorption, when mixed with atmospheric air or any other gas, is not the same as that with pure chlorine alone. Dr. Fyfe has either not tried this, or has entirely overlooked it; and it is as great an oversight as that which he has sought to cor-

rect. I have made a great number of experiments to determine this point. A quotation or two will serve to show how fallacious the method is.

1. Passed 2·9 cubic inches of nearly pure chlorine, collected over water that had been boiled to expel atmospheric air, into a graduated eudiometer standing over water at 50° Fah. The gas measured 1·9 cubic inch. After standing 15 minutes, the gas measured 1·68 cubic inch. This indicated an absorption amounting to 22 parts.

2. Passed equal measures of chlorine and atmospheric air into the same eudiometer. The mixed gases measured 1·9 cubic inches. After the lapse of 15 minutes, the mixed gases measured 1·8 cubic inch. The absorption, therefore, in the same time and under the same circumstances as in No. 1 explanation, was only 10 parts.

The rate of variation is not constant. It varies with the purity of the chlorine itself, and with the quality and proportion of the gas added to it. Besides, as I shall prove further on, the assumption by Dr. Fyfe, that the gases condensed by chlorine consist entirely of olefiant gas, is not correct; and therefore, even if the objections to the method just stated did not exist, it could only afford a crude approximation to the composition of the gas, and to its relative illuminating power.

A better method of determining the amount of olefiant gas, and which was employed by Dr. Henry, and is recommended in some of the best analytical works, is to allow the chlorine and gas to re-act on each other, and then to remove the whole of the chlorine by an absorptive solution, determining the amount of olefiant gas by the diminution in bulk of the original quantity employed. This method I have subjected to a very rigid examination. Pure chlorine is recommended to be employed; but it has been overlooked that it is almost impossible to obtain pure chlorine over water, and mercury absorbs it so rapidly that experi-

ments cannot be performed over this metal with chlorine. I have prepared chlorine from perfectly pure material, in vessels completly filled with fluid so as to exclude all air, and collected the gas over boiling water; and still, on acting upon it by an absorptive solution, there was always a residuum left. I collected the residuum from several operations, and analysed it. I found it to be composed entirely of atmospheric air. Unless the exact proportion of this be accurately determined, it will vitiate the results, as in Dr. Fyfe's experiments, because it must be allowed for, which has not generally been done. But, unfortunately, the proportion of this unabsorbable residue varies with every portion collected; and, therefore, the examination of one bottle of chlorine does not give the true amount of impurity to be allowed for in that employed for the analysis. From the same quantity of gas (chlorine) I obtained in one instance, ·625; in another, ·637; in another, ·03, as the amount of residuum. I was long puzzled as to the source of this impurity; but at length found it to proceed from the air, mechanically retained or absorbed by the water and other fluids employed in the processes. This was displaced by the more absorbable chlorine, and hence its constant presence in the unabsorbed chlorine, and the constant variation in its quality. Of late, Professor Bunsen of Marburg, finding the difficulty of truly determining the amount of olefiant gas by the means hitherto employed, has proposed and adopted the use of charcoal or coke balls, saturated with fuming sulphuric acid, for the removal of the olefiant gas. This process fully answered for the separation of the illuminating gases; but the experiments, and the whole of the subsequent analysis of the residual gas, have to be performed over mercury, and require great care in the manipulations; still it is by much the best hitherto proposed. The best method of determining the general value of the gas is also due to the late Dr. Henry, and is the one which I have

generally employed in the analysis of our own gas, and that of the neighbouring towns. In this process, the relative value of the different gases is determined by the quantity of oxygen required to effect the complete combustion of the gas. This is done by firing the mixed gases by electricity in graduated tubes, and calculating the oxygen consumed in the production of carbonic acid and water from the gas.

By a combination of these two methods I have arranged a plan of analysis, by which the general value as well as the constitution of any coal gas can be determined with accuracy, and the illuminating gases can receive an expression sufficiently high to indicate even small amounts of differences in their proportions.

I first determine, by an analysis with oxygen alone, the number of volumes of oxygen required for complete combustion, by 100 volumes of the gas to be examined, and the quantity of carbonic acid produced; I then, from another portion of the same gas, withdraw the olefiant and other illuminating gases by fuming sulphuric acid, and determine the exact amount of these; I then subject a portion of the residual gas to examination with oxygen again, and determine the number of volumes required; the difference in the two examinations gives the amount of oxygen required by the illuminating gases removed by the sulphuric acid, and of carbonic acid produced. In the second portion of the residual gas I determine the quantity of each constituent gas, and estimate the amount of each in the whole mixture.

This is accomplished in the way generally practised, viz., by exploding the residual gas with oxygen, determining the amount of oxygen consumed, and of carbonic acid produced; and from these data, calculating the proportions of light carburetted hydrogen, hydrogen, carbonic oxide, and nitrogen.

I append an analysis made, after the manner described above, of gas from a cannel much used at the Manchester gas-works.

100 volumes of Ince Hall (Wigan) cannel gas required for complete combustion 146·5 volumes of oxygen; 100 volumes of the same gas, being treated with anhydrous sulphuric acid, lost 8·5 parts, consisting of olefiant and other illuminating gases.

The residual gas required 104 volumes of oxygen for complete combustion. So that the 8·5 parts which had been removed by the sulphuric acid, had required 42·5 parts of oxygen for combustion. This gives the number 5 as the expression in volumes of oxygen of each volume of the gas removed by sulphuric acid; and to these 8·5 parts are due the chief portion of the illuminating power of the coal gas.

This also proves what I before referred to, that the illuminating gas in coal gas does not consist of olefiant alone, as each volume of this gas requires only 3 volumes of oxygen for combustion; it is a mixture of olefiant gas with what I would call trito and tetarto-carburetted hydrogen. It is evident that, with so large a multiple as 5 for each volume of illuminating gases, even small differences of the latter in any sample of gas can be correctly indicated and expressed.

Below is the composition of the particular sample of gas taken in illustration.

Ince Hall (Wigan) Cannel Gas.

| | |
|---|---|
| Carbonic Acid | 0·78 |
| Olefiant Gas and Illuminating Hydro-carbons represented by 42·5 vols. of Oxygen | 8·50 |
| Atmospheric Air | 4·32 |
| Nitrogen | 0·19 |
| Hydrogen | 41·00 |
| Light Carburetted Hydrogen | 33·83 |
| Carbonic Oxide | 11·35 |
| | 99·97 |

I have made complete analyses of the gas from almost every considerable town in England and Scotland, and have examined analytically the gas from the greater number of coals and cannels; and shall be glad on a future occasion, if not objected to by the Manchester Gas Committee, to lay the results before the Society.

## XXI.—*A Description of some supposed Meteorites found in Seams of Coal. By* Mr. E. W. Binney.

Read May 13, 1851.

The component parts of sedimentary rocks afford the geologist most valuable data in assisting him to arrive at an estimate of the forces which have been in operation on the earth's surface in very remote ages. Accordingly, we find that the earliest cultivators of geology paid considerable attention to the conglomerates, sandstones, and slates of the older deposits, as well as to the gravels, sands, and clays of more recent formations. In a paper read by the author before this Society on the 1st day of December, 1846, and printed at p. 148 of vol. viii. (new series) of the Society's *Transactions*, the mechanical deposits of the coal-measures of Lancashire were investigated at some length, for the purpose of attempting to measure the intensity of the currents of water which brought them to the places where they are now found. At p. 166 is the following extract: "As before stated, rough gritstones, containing rounded pebbles of quartz, abound in the lower coal field; whilst the middle and upper measures, reaching to a thickness of 4,472 feet, as far as I know, have never yet afforded a piece of mineral matter, in their sedimentary deposits, of the size of a small pea. In two *seams* of coal, namely, the Four Feet Mine at Patricroft, and another seam under the same mine at Pendleton, I have obtained rounded stones of several pounds in weight; but as both these specimens came from the neighbourhood of great faults, probably

they may have been brought to the places where they were found by other causes than currents of water. They, however, are interesting, and difficult to account for, being well rounded. Their composition is the same, though found in different seams and at different places, being of a hard crystalline quartz, more resembling gannister than any other stone in the carboniferous series. The outsides of both stones are well coated with a covering of coal, shewing that they must have lain long in the places where they were found."

Ever since the reading of the above paper, I have devoted considerable time and trouble in attempting to obtain evidence of more stones having been found in coal seams—of course, by stones I don't mean any of those aggregations of iron pyrites and ironstone which are so frequently met with in coal seams, but foreign masses of stone, which must have been introduced into the coal when it was in a soft state, and not precipitations from water, or segregations from the substance of the coal itself, where they had previously existed either in solution or admixture. All my enquiries, however, resulted in obtaining no proof of more specimens having been found in coal seams except the one next alluded to.* In the *Mining Journal* of the 9th day of November, 1850, appeared the following paragraph: "A large pebble of crystalline or primary limestone,† was found imbedded in the solid coal at the Rhydgaled Colliery, near Mold, on Monday the 4th instant. It is supposed to be

* Since this paper was read, the author has had an opportunity of asking W. E. Logan, Esq., F.R.S., director of the geological survey of Canada, a gentleman of as great practical acquaintance with coal fields as any geologist of the day, and one who has investigated coal-measures in nearly all parts of the world, if he ever, in his great experience, had met with rounded pebbles of stone in the middle of coal seams, and that gentleman declared that he had not met with a single instance—E. W. B.

† This stone, as will be seen by the analysis hereinafter given, is not a limestone, but nearly pure silica.

the first instance known of such a pebble having been found in the coal strata." This convinced me more than ever that such stones were of rare occurrence, especially as none of the readers of that journal, which has an extensive circulation amongst the practical coal-miners of Great Britain, stated in its pages that any such pebbles had come under their observation.

In answer to a letter addressed to Mr. Edward Jones, a gentleman who has the management of the Rhydgaled colliery, in March, 1851, the stone was liberally sent to me for examination, with a consent to analyse it, accompanied by the following letter:—"The stone was found by a person of the name of Edward Price, on the 4th November, 1850, whilst hewing the coal. It was imbedded in the upper part of the coal, within ten inches of the top of the seam, in a part of it called bone coal from its extreme hardness. The layers of coal that surrounded it were perfectly regular; so that, had the stone been immersed in a vessel containing metal in a fused state, and allowed to remain there until it was cooled, it could not have been more accurately fitted in its place. The seam in which it was found is called the main coal, and is the lowest that has been discovered in this neighbourhood. It is superior in quality to any other seam in the formation, and is the one most extensively worked here. Perhaps I should state, that the place where the stone was found was within twenty-five yards of a fault of considerable size."

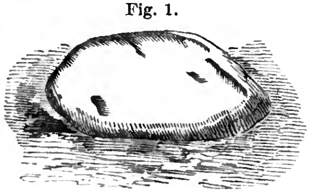
On comparing the Welsh stone with the two specimens of stones found in the Lancashire coal seams in my possession, their great resemblance in characters induced me to attribute them to a common origin, and to endeavour to find out what that origin was. My attention has therefore been directed to this enquiry; and, although considerable time has been spent in hunting after more specimens of stones found in coal seams, no further information has been obtained of

the occurrence of any. Descriptions will now be given of the stones in my possession.

THE PENDLETON SPECIMEN.

This was found by Mr. Andrew Ray, the intelligent manager of the colliery of the Pendleton Coal Company, in the year 1839, in sinking the new pit there. It was met with in the middle of the 6-feet seam of coal, at a depth of 245 yards from the surface. Mr. Ray, thinking it a great curiosity, brought it to me. At first I did not pay much attention to the specimen, thinking it was merely some boulder stone which had been squeezed into the coal from the great Irwell fault, which is not more than about 50 yards from the place where the specimen was found. On more careful examination, the external characters as well as the composition of the stone, however, soon led me to consider it unlike any stone that ever previously came under my notice. This specimen is composed of a crystalline quartzose stone of a light gray, with mottled marks of a black colour, and containing small crystals of sulphuret of iron dispersed through the body of the stone. Its outside is moderately smooth, with traces of slickenside, as if it had been subjected to considerable pressure. The colour is dark black, with a slight polish on the stone, and some portions of a substance like the pulverulent carbonaceous matter, so commonly found in coals adhering to it. The black coating is a remarkably thin one on the outside of the stone, without penetrating scarcely at all into it.

Fig. 1.



Its form (see fig. 1) is that of an irregularly compressed oval, having one of its ends a little pointed, 5 inches in length by $3\frac{1}{4}$ inches in breadth. It has a specific gravity of 2·58, and weighed about $2\frac{3}{4}$ lbs. avoirdu-

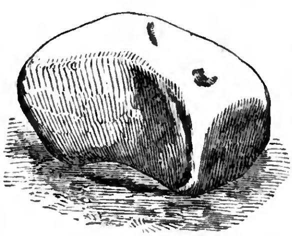
pois when whole. By the kindness of my friend, Dr. Robert Angus Smith, I am enabled to give an analysis of it, which is as follows:—

| | |
|---|---|
| Silica | 96·463 |
| Alumina | 2·578 |
| Protoxide of Iron | ·644 |
| Lime | ·161 |
| | 99·846 |

THE PATRICROFT STONE.

This was found by Mr. John Smith, in the colliery of Messrs. John Lancaster and Co., at Patricroft, near Manchester, in the 4-feet seam, at a depth of 440 yards from the surface, about the year 1845. It is composed of a crystalline quartzose stone of a darker gray than the specimen last described, and having very small black spots dispersed through its mass. The outside is partly smooth and partly irregular, of a shining black polish on the surface, but scarcely penetrating at all into the body of the stone. Marks of slickenside are seen on all parts of it, with several patches of sulphuret of iron.

Fig. 2.



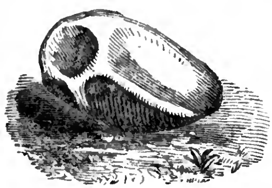
Its form (see fig. 2) is that of an irregular pyramid, having most of its angles rounded off. The greatest length is 7 inches, and the breadth 4 inches. It weighs 6½ lbs. avoirdupois, and has a specific gravity about 2·60. Dr. Smith's analysis of it is as follows:—

| | |
|---|---|
| Silica | 96·050 |
| Alumina | 2·529 |
| Protoxide of Iron | ·709 |
| Lime | ·525 |
| Magnesia | ·124 |
| | 99·937 |

THE RHYDGALED STONE.

This, as previously mentioned, was found in November, 1850, in the main seam of coal at Rhydgaled, near Mold. It is composed of a crystalline stone of a grayish-white, with some small streaks and spots of a black colour dispersed throughout the mass. The outside is generally smooth, but contains a few little holes on its surface. It is coated with a shining black polish, just like a thin varnish, without penetrating far into the body of the stone, as in the two last specimens. There are marks of slickenside on the outside, but not so strong as those on the specimen from Patricroft.

Fig. 3.



Its form (see fig. 3) is that of an irregular oval, with three of its sides and one end compressed. The greatest length is $5\frac{1}{4}$ inches by $2\frac{3}{4}$ inches in breadth. It weighs $1\frac{3}{4}$ lb. avoirdupois, and has a specific gravity of 2·60. Dr. Smith's analysis of it is as follows:—

| | |
|---|---|
| Silica | 99·182 |
| Alumina | ·649 |
| Protoxide of Iron | ·022 |
| Lime | ·016 |
| | 99·869 |

All the three stones were found in seams belonging to the middle division* of the coal field, the two first named in the higher, but the last named in the lower portion of it. Had they been found in the rough gritstones of the lower coal field, where most of the sandstone rocks prove that considerable currents of water had been in action; and that even some of the seams of coal, especially one known by the name of the Feather Edge Coal in Lancashire, are sometimes

* For the definition of this part of the coal field, see *Transactions of the British Association for the Advancement of Science*, vol. xii. p. 46; and Sturgeon's *Annals of Philosophical Discovery*, and *Monthly Reporter of the Progress of Practical Science*, vol. i. p. 126.

found to have been wholly or partially removed by the effects of running water, it would not have been very remarkable; but when these stones are found in the midst of the most tranquil deposits of the whole series, with no trace of a portion of rolled mineral matter of the size of a pea for thousands of feet in vertical height, their occurrence, in the places where they were found, is difficult to account for.

Sir Charles Lyell, at page 217 of the last edition of his *Elements of Geology*, in speaking of the pebbles in the chalk, states as follows:—"The general absence of sand and pebbles in the white chalk, has been already mentioned; but the occurrence here and there, in the east of England, of a few isolated pebbles of quartz and green schist, some of them two or three inches in diameter, has justly excited much wonder. If these had been carried to the spots where we now find them, by waves or currents from the lands once bordering the cretaceous sea, how happened it that no sand or mud was transported thither at the same time? We cannot conceive such rounded stones to have been drifted like erratic blocks by ice, for that would imply a cold climate in the cretaceous period,—a supposition inconsistent with the luxuriant growth of large-chambered univalves, numerous corals and many fish, and other fossils of tropical forms.

"Now, in Keeling's Island, one of those detached masses of coral which rise up in the wide Pacific, Captain Ross found a single fragment of greenstone, where every other particle of matter was calcareous; and Mr. Darwin concludes, that it must have come there entangled in the roots of a large tree. He reminds us that Chamisso, the distinguished naturalist who accompanied Kozebue, affirms that the inhabitants of the Radack archipelago, a group of lagoon islands in the midst of the Pacific, obtained stones for sharpening their instruments, by searching the roots of trees which are cast up on the beach.

"The only other mode of transport which suggests itself is seaweed. Dr. Beck informs me, that in Lym-Fiord in Jutland, the *Fucus vesiculosus,* often called Kelp, sometimes grows to the height of ten feet; and the branches rising from a single root form a cluster several feet in diameter. When the bladders are distended, the plant becomes so buoyant as to float up loose stones several inches in diameter, and these are thrown by the waves high up on the beach. The *Fucus giganteus* of Solander, so common in Terra del Fuego, is said by Captain Cook to attain the length of 360 feet, although the stem is not much thicker than a man's thumb. It is often met with floating at sea, with shells attached, several hundred miles from the spots where it grew. 'Some of these plants,' says Mr. Darwin, 'were found adhering to large loose stones in the inland channels of Terra del Fuego, during the voyage of the Beagle in 1834; and that so firmly, that the stones were drawn up from the bottom into the boat, although so heavy that they could scarcely be lifted in by one person.'"

No doubt there is a far greater abundance of fossil trees in the coal-measures than in the chalk; but still there is little evidence to show that it is at all probable that the stones found in coal seams had been carried to the places where they are now met with in the roots of trees, any more than that the trees themselves were drifted. Doubtless a Sigillaria, having immense stigmariæ roots, with radicles radiating from them in all directions to a great length, would be as likely a root as could be desired for the purpose of conveying a stone. But where and how is the Sigillaria to get loaded with its burden? This is a difficult question to answer. The plant, of which this remarkable fossil is the root, must have grown beyond all question in soft mud, and not on a rocky bottom; and, even if it had grown in the latter position, coal seams, in Lancashire at least, as I have shown in a paper printed in the last

volume of the *Transactions* of this Society, bear no evidence of the vegetables composing them having been drifted, but, on the contrary, show that they were grown where they are now found, the seam of coal being simply a mass of altered vegetable matter, lying upon a bed of tree roots, and having stems of similar trees resting upon it. A seam of coal like those in which the stones were found, bears no more evidence of a current of water than an ordinary peat bog does, and a rolled stone is just as likely to be found in the middle of the one as in the other, if we admit that the vegetable matter now constituting coal, grew on the spot where it is found. In the bog, over which the Liverpool and Manchester Railway now passes, known by the name of Chat Moss, are some pits containing water called ringing holes. The people residing near the moss have a tradition, that if any one can find a stone on the bog which has not been brought from a distance, and throws it into the holes, it will ring like a church bell. But this interesting experiment has not yet been tried, from the simple reason that no one, up to this time, has yet been able to discover such a stone!

If, therefore, it is difficult to account for the occurrence of the stones, previously described in this paper as found in seams of coal, being conveyed to the places where they were met with by the action of running water, we must look to some other cause for their origin.

The shape of the stones is not such as we should expect to have been precipitated from solution in the water in which the vegetable matter was immersed, like the flints in chalk. Nor does their size allow of any probability of their being secreted from the sap of plants, like the crystals of silica, which are sometimes met with in the sugar-cane and some other plants. The trees of the carboniferous series have, without doubt, been of a most extraordinary character when compared with those at present existing; but still we cannot

for a moment imagine even that they were capable of producing in their insides stones similar to those described in this paper.

Now, in my humble opinion, there is another cause to which we can attribute the position of the stones in the seams of coal in which they were, without doubt, found, by supposing that they are meteorites which fell from the atmosphere, and became imbedded in the coal when it was in a soft state, and before it was covered by the overlying roof.

Up to this time, few meteorites have been found in the strata composing the crust of the earth. In note 83 of Lieutenant-Colonel Sabine's translation of Baron Humboldt's *Cosmos*, is the following passage:—"Olbers acutely observes, that it is a remarkable circumstance, not hitherto noticed, that no fossil meteoric stones have as yet been found, like fossil shells, in secondary and tertiary formations. Are we to infer that, previous to the last and present arrangement of the surface of our planet, no meteoric stones had fallen upon it; although, according to Schreibers, it is probable that 600 falls of aerolites now take place in each year?—(Olbers in *Schum. Jahrb.*, 1838, S. 329.) Problematical nickeliferous masses of native iron have been found in Northern Asia, at a depth of 31 French feet, and recently among the Carpathian mountains; both these masses are very like meteoric stones."

Sir Charles Lyell, in the third edition of his *Manual of Elementary Geology*, at page 145, alludes to the first-named mass of native iron above mentioned, and as having been found in the alluvium at Petropawlowsker in the Mrassker circle with more confidence. He, however, states that no sufficient data are supplied to enable us to determine whether it be of post-pliocene or newer pliocene date. He further adds—"We ought not, I think, to feel surprise that we have not hitherto succeeded in detecting signs of

such aerolites in older rocks; for, besides their rarity in our own days, those which fell into the sea (and it is with marine strata that geologists have usually to deal), being chiefly composed of native iron, would rapidly enter into new chemical combinations, the water and mud being charged with chloride of sodium and other acids. We find that anchors, cannon, and other cast-iron implements, which have been buried for a few hundred years off our English coast, have decomposed in part or entirely, turning the sand and gravel which enclosed them into a conglomerate, cemented together by oxide of iron. In like manner meteoric iron, although its rusting would be somewhat checked by the alloy of nickel, could scarcely ever fail to decompose in the course of thousands of years, becoming oxide, sulphuret, or carbonate of iron, and its origin being then no longer distinguishable. The greater the antiquity of the rocks—the oftener they have been heated and cooled, permeated by gases or by the waters of the sea, the atmosphere or mineral springs—the smaller must be the chance of meeting with a mass of native iron unaltered; but the preservation of the ancient meteorite of the Altai, and the presence of nickel in these curious bodies, renders the recognition of them in deposits of remote periods less hopeless than we might have anticipated."

In the translation of Humboldt's *Cosmos*, before cited, at page 118 of Vol. I. is the following passage, which, as it contains valuable information on the subject before us, will be given at length :—"The solid masses which reach the earth—whether they have been seen to fall at night from balls of fire, or in the daytime from a small dark cloud, usually in a clear sky, and with a loud noise—though considerably heated, are not incandescent. They exhibit, on the whole, a general unmistakeable resemblance to one another in their external form, in the nature of their crust, and in the chemical composition of their principal constitu-

ents; and this resemblance is traceable, when and wherever they have been collected, at all periods of time, and in all parts of the earth. But this remarkable and early recognised similarity of general character in solid meteoric masses, suffers many exceptions in detail. How different are the very malleable masses of iron from Hradschina, in the district of Agram; or those from the banks of Sisim, in the Jeniseisk government, mentioned by Pallas; or those which I brought from Mexico—all of which contain 96 per cent. of iron—from the aerolite of Sienna, which hardly contains 2 per cent. of iron; from the earthy meteoric stone of Alais, in the Department du Gard, which falls to pieces when immersed in water; and from those of Jonzac and Juvenas, which are without any metallic iron, and are composed of various crystalline ingredients? These diversities have led to the division of the cosmical masses under consideration into two classes—nickeliferous meteoric iron, and fine or close-grained meteoric stones. The crust of these masses, which is only a few tenths of a line in thickness, is very characteristic; it has often a pitchy lustre,* and is sometimes veined. The only instance which I know of the absence of this crust, is in the meteoric stone of Chantonnay in La Vendee, which is marked by another circumstance equally rare, viz., the presence of pores and vesicular cavities, like the meteoric stone of Juvenas. The separation of the black crust from the light grey mass beneath, is always as sharply defined as in that of the dark leaden-coloured crust of the white granite blocks which I brought from the cataracts of Orinoco, and which are also found by the side of many cataracts in other parts of the world, as those of the Nile and the Congo. The greatest heat of our porcelain furnaces can produce nothing similar to the crust of the aero-

* Pliny has remarked the peculiar colour of the crust of aerolites "colore adusto" (11, 56 and 58). The expression "lateribus pluisse" also refers to the burnt appearance of the exterior.

lites, so distinctly and sharply separated from the unaltered mass beneath. Appearances which might seem to indicate a softening of the fragments, have been occasionally recognised; but, in general, the condition of the greater part of the mass—the absence of any flattening from the effects of the fall—and the moderate degree of heat perceived on touching the newly-fallen aerolite—are far from indicating a state of internal fusion during its rapid passage from the limits of the atmosphere of the earth."

The chemical composition of the three stones previously described in this communication, undoubtedly shows less iron than exists in the majority of meteoric stones hitherto examined. The composition of the Waterloo stone, found in Seneca county, New York, and described by Professor Shepard in his Report on Meteorites, at page 40, No. 31, Vol. XI. of *Silliman's Journal*, has some analogy in its composition and specific gravity to the stones now under consideration. The analysis of this stone gave,

| | |
|---|---|
| Silica | 78·80 |
| Peroxide of iron | 8·72 |
| Alumina | 6·28 |
| Moisture | 4·75 |
| Lime and magnesia, and loss | 1·45 |
| | 100·00 |

Specific gravity 2·30.—The Waterville and Concord stones, also described by Professor Shepard at pp. 414 and 416 of No. 18 of Vol. VI. of *Silliman's Journal*, contain no iron, but a large quantity of magnesia. According to Dr. Shepard, the first five chemical elements thus far known to exist in meteoric masses, in the supposed order of their prevalence, are as follows:—Iron, nickel, magnesium, oxygen, and silicon, p. 386, Vol. II. No. 6 (second series), of *Silliman's Journal*. For a long time scarcely any meteorites were recognised as such, which did not contain a large amount of metallic iron; but, now attention has been directed to

these bodies, many without iron will doubtless be met with. So there is nothing in the composition of the stones described in this paper to prevent them from being considered as meteoric, even supposing that such bodies which fell to the earth in so remote an age as the carboniferous strata, were exactly similar in their nature to those which visit the earth at the present time—a supposition not very probable.

Scarcely any of the strata of the earth, in England at least, have been so thoroughly explored as the valuable seams of coal, which have contributed so largely to our national resources; and therefore it is in those strata that we should certainly look with the greatest probability for finding ancient meteoric stones. Also, as Sir Charles Lyell has observed in the preceding quotation, in his remarks on fossil meteorites, it is not the masses of iron which fell in the waters of the ancient globe that we should expect to find preserved in the strata, but rather such stones as we have previously described, consisting nearly altogether of silica, and, therefore, capable of resisting the decomposing agents, which metallic iron, and many other bodies, would be nearly incapable of enduring. The rare occurrence of such bodies as the stones before described in seams of coal, may, with propriety, be adduced in attributing them to some extraordinary cause rather than the effects of currents of water, and their transport by the roots and branches of trees.

The shape of the three specimens, the specific gravities and chemical composition, do not prove much either for or against their being considered as meteorites; for if the seams of coal in which they were found had been formed of drifted trees, as was formerly the favourite hypothesis for accounting for the origin of coal, the stones might be taken for travelled pieces of quartzose stone, equally with the drifted vegetables; but each of the seams in which they occurred (and they were found in the middle of the beds), is placed on

a floor full of stigmariæ roots, thus affording conclusive proof that the plants of which the seams were formed grew upon the spots where they are now found.

The crust of the specimens, consisting of only a few tenths of a line in thickness, of a shining black lustre, and so sharply defined from the light gray mass of the stone beneath, is the strongest evidence of their being meteorites. This is the peculiar character of meteorites, so forcibly alluded to by Humboldt in the quotation previously given. The black colour of the stones might certainly have been derived from the decomposing vegetable matter in which they have been so long enveloped; but it is very remarkable that the colouring matter should have penetrated so slightly into the body of the stone. An ordinary pebble of quartz, after only a few years' immersion in black mud, composed of decaying vegetable matter, would be much more discoloured in depth than the specimens in question, which have lain for countless ages; and the present slight coating of the latter can only be attributed to their outsides having been subjected to the action of great heat, and thus so vitrified as to prevent the colouring matter from entering into the body of the stones before they came amongst the vegetable matter now forming coal.

Nothing is more difficult than to establish, without question, the meteoric character of a stone. For ages doubts were thrown even on those specimens which were seen to fall from the heavens, and were actually found in a hot state. With fossil meteorites still greater difficulties occur; and the specimens described in this paper can only be considered as such bodies, by their resemblance in their characters to recent meteorites, and by their being found in a position where it is more probable to suppose they came through the atmosphere, than by currents of water, or any other ordinary cause.

XXII.—*On the Volvox Globator.* *By* WILLIAM CRAWFORD WILLIAMSON, *Professor of Natural History in Owens College, Manchester.*

Read May 27, 1851.

FROM the long period that has elapsed since observers first became interested in the *Volvox globator* as a microscopic object, it is a matter of some surprise that its structure should still be so imperfectly understood. Such, however, is really the fact. The idea of its unity as an individual animal having been rejected by Professor Ehrenberg, many naturalists adopted the more novel interpretation of its history, enunciated by the illustrious Prussian, who was almost universally considered to have dispelled the obscurity with which this elegant organism was previously invested. Whilst all honour is acknowledged to be due to this great observer for the brilliant results of many of his labours, it has become manifest, that in points relating to the internal structure and physiology of some of the objects which he has investigated, his conclusions require to be received with a degree of caution. It is now known that he has included in his great work on infusorial animals, a large number of plants, to which he has assigned organs and functions which are really confined to animal life.

The consequence has been, that doubts have gradually suggested themselves as to how far his conclusions are to be relied upon, in reference to many of the other objects delineated in his magnificent volume. The *Volvox globator* has come in for a share of this scepticism, and not without reason. But whilst many have disputed the accuracy of Ehrenberg's interpretation of its structure and history, I

am not aware that any other observer has grappled with the subject in order to supply the deficient knowledge.

Having recently met with large numbers of this beautiful object in a pond near Rusholme, I have subjected it to a careful and protracted examination, in the hope of clearing up some of the points considered to remain unsettled.

As is well known, Professor Ehrenberg regards each of the small green specks with which this organism is studded, as a distinct polygastric animalcule, having an oral orifice leading to several stomachs, an eye, organs of generation, and divergent canals, which are supposed to maintain a communication between it and the individuals by which it is surrounded. According to this view, the *Volvox* consists of an association of similar individual animals, uniting to form a hollow sphere, a structure somewhat analogous to that of the zoophytic polyparies.

Though this interpretation has recently received the sanction of Professor R. Jones (*Encyclopædia of Anatomy and Physiology, Article Polygastrica, Nov.* 1847) and is also adopted by some other leading naturalists, I confess I cannot reconcile it with the appearances which the careful use of a good microscope reveals. The details of its structure and development appear to exhibit less affinity with the phenomena of animal than of vegetable life. Regarding it as a plant, we have comparatively little difficulty in understanding its history; but it is no easy task to bring it into accordance with our ideas of what is essential to animal life, modified though these have been within late years. Having little doubt in my own mind, that it is a true Confervoid plant, I will proceed to explain the details of its structure, in accordance with this general conclusion.

Commencing with the examination of a very young individual, we find that it is a hollow vesicle, the walls of which consist of numerous angular cells (*Fig.* 1 *a*), filled with green endochrome, mixed with minute granules, the

intercellular spaces being more or less transparent. After a while, the green colouring matter changes its form, losing its regularly angular aspect, and assuming the appearance of *Fig.* 2 *a.* In this stage we cease to perceive the original outlines of the cell; but on the application of some re-agents they can readily be brought into view. This irregularity of form appears to arise from the existence of a ductile cell-membrane which primarily lines the interior of each cell, whilst it surrounds the cell-contents. This inner membrane becomes separated from the outer cell wall, excepting at a few points (*Fig.* 2 *b*), where it is retained in contact. It now undergoes some curious changes, which alike affect its form and the character of its contents.

After a time the angular projections of the inner membranes of different cells appear to become still more intimately united, as represented in *Fig.* 3 *a.* This appearance seems to be caused by the further diminution of the connection between the outer cell-wall and its internal cell or lining membrane, from the shrinking of the latter tissue.

Whilst these changes are in progress, an increase in the diameter of the entire organism is taking place, unaccompanied by any corresponding addition to the number of the cells, or to the amount of endochrome which they contain. Hence the older organisms appear much more transparent than the younger ones. Along with this general enlargement, there appears to be an increase in the diameter of the individual cells; but whether this is owing to a real distension of the outer cell-membrane, or merely to the flattening of cells which were previously somewhat spherical, is doubtful. I suspect that the former may be the case. It is obvious that the superficial area of each cell enlarges; and as the projecting radii of the inner cell and its endochrome retain their attachment to the outer cell-wall, the expansion of the latter leads to an elongation of the former; this is, of course, accomplished at the expense of the central

mass of lining membrane and endochrome, which diminishes as the radiating processes become elongated.

Owing to these changes, the green cell-contents assume a stellate form (*Fig.* 4 *a*), and the intermediate transparent spaces become proportionately enlarged. This expansion of the cell-wall, and elongation of the thread-like processes of the lining membrane, go on until we have the appearance presented by *Fig.* 5, in which state the central green cell (*Fig.* 5 *a*) is seen to be much reduced in size, as well as altered in shape—having become more triangular; whilst the processes (*Fig.* 5 *b*) attaching it to its outer cell-walls, are elongated, and often branched. In this stage it is very rarely possible to trace the outlines of the original hexagonal cells. I have, however, been able to identify them sufficiently often to establish their existence. By rupturing a *Volvox* under water containing a slight trace of tincture of iodine, and at the same time paying great attention to the management of the light, I have seen them with great distinctness, as represented by the faint lines, *Fig.* 5 *c;* each one containing its own endochrome. The projecting threads which maintain the inner cell in its position (*Fig.* 5 *b*), and which are usually attached to the cell-walls at points exactly opposite the corresponding processes of adjoining cells, give the whole the appearance of continuous canals, connecting together the separate masses of endochrome. It is in this light that they were regarded by Ehrenberg, who appears never to have seen the hexagonal cells within which they are enclosed, and the thin cell-walls of which intervene between the opposite extremities of apparently continuous threads.

During the progress of these transitions from the angular to the stellate form, corresponding changes are affecting the character of the other cell-contents. In the early stages of growth, each cell, as already stated, contains an abundance of dark green endochrome, along with numerous minute

granules of an uncertain nature, but apparently analogous to those seen in so many of the fresh-water Algæ. After a while, a single large green granule (*Fig.* 3 *and* 4 *b*) makes its appearance in each cell, whilst many of the minute ones, previously observed, disappear. One or two of the latter, however, not only remain unabsorbed, but continue to enlarge; and from their pale colour, and high refracting power, become very brilliant (*Fig.* 5 *a and* 12 *a*). The large green granule now disappears in its turn, whilst a new feature becomes increasingly conspicuous: this is the celebrated red spot which Ehrenberg regards as the eye of his animalcule. For some time I thought that the latter object was the direct result of a transmutation of the former granule, so frequently did the appearance of the one mark the disappearance of the other; but this is not the case. They are perfectly distinct, though the red speck is rarely to be seen when the green granule is fully developed, or even beginning to be absorbed. On the disappearance of the latter object, we find that the remaining green endochrome has assumed a paler hue; but the one or two brilliant granules (*Fig.* 5 *f*, 12 *a*) just referred to, have materially increased in size and conspicuity, occupying a large proportion of each green central area. In fact the only contents of the latter, in the present stage, are the granules, the red spot, and a small quantity of pale green homogeneous endochrome.

The brilliant points, which are the generative organs of Ehrenberg, are perfect and well-defined spheres; but I have not been able, by any adjustment of the instrument, to obtain so definite an outline to the eye spot (*Fig.* 5 *d and* 12 *b*) of the Prussian naturalist. It appears to be the result of an alteration in the condition both of the lining membrane, and of some of the granular cell-contents which adhere to it. The degree of its distinctness is very variable. In some specimens its presence is barely to be detected, even in the most advanced condition of the cells.

Implanted above each of the green areas, we have two ciliæ, or "proboscides" of other writers (*Fig.* 5 *e, and* 12 *c*), to which further reference will be made.

On rupturing a *Volvox* between two glasses moistened with water, we find ample confirmation of the accuracy of the above views. By the bursting of the organism, a number of cells along the torn margins are laid open. The cell-contents of the cells whose outer walls are so ruptured, instantly lose their stellate form and become spherical (*Fig.* 4 *d and* 12); a result produced by the liberation of the elongated processes from their points of attachment to the cell-walls, and which is probably owing to the existence of some elasticity in the inner cell-membrane.

But whilst many of the cell-contents, thus altered in their contour, escape from the cavities of their respective outer cells, individuals are sometimes retained in connection, by means of a long thread, as at *Fig.* 4 *d and e*. It is evident that the whole of the cell-contents, with the exception of the brilliant spherical granules, are remarkably ductile and cohesive. They may be drawn out into long threads to the extent of several times their own diameter (*Fig.* 4 *e*). This singular viscid character readily accounts for the extension of the stellate processes, on the enlargement of the areas of the individual cells. It is evidently the property which has caused the elongation of the threads at the expense of the central mass, and is possibly owing to the existence of a large quantity of gummy matter in their substance. In a little time after water has obtained access to the interior of the *Volvox*, we find that an analogous change gradually steals over the contents of all the other cells. The processes become successively detached from the cell-wall, and are drawn in towards their respective centres, which also become globular (*Fig.* 12). For a time the cell-contents, though thus altered in form, do not escape from the cells in which they are contained,—as we found

to be the case when the cell-walls had been torn across; but after a while many of the cell-walls appear to become so far softened as to allow the cell-contents to float out. When this is the case, the latter objects invariably pass into the *interior* of the *Volvox*. They never appear to break through the outer wall. On examining the cell-contents, after they have thus made their escape, we find that they have undergone no change beyond that of external form: their composition is the same as when seen *in situ*, according to the degree of development which the individual under examination has undergone. When thus liberated, they exhibit no traces of the two ciliæ or "proboscides" of Ehrenberg, and which he describes as belonging to the individual "animalcule," and being merely projected through the investing membrane. Neither have we any thing resembling the oral aperture, or sacculated stomachs, delineated in his figure. Indeed, the whole appears exactly like the ordinary cell of an Ulva, deprived of its external cell-wall.

On turning our attention to the external membrane, from the under surface of which these objects have escaped, we find that the ciliæ have been left behind, and are implanted in pairs upon its surface, in a very regular order (*Fig.* 6). We observe a series of small specks arranged in pairs, and which preserve a degree of relative parallelism (6 *a*); from each of these points a long cilia is projected. It has already been remarked, that when the cell-contents have escaped from a ruptured cell, the vesicle is frequently retained in connection with the latter, by means of a delicate ductile thread (*Fig.* 4 *e*). In this case the thread always terminates at one or both of these small specks, indicating a more intimate union between the cell and its contents at these points than at any other. It is not easy to say what these specks are. They do not appear to be merely minute apertures, since they exhibit a power of condensing transmitted light.

For a short time after the rupture of the *Volvox*, the filamentous ciliæ continue their active whip-like movements; but these gradually cease, and soon afterwards the filaments detach themselves from the membrane in great numbers, floating away into the surrounding medium.

I was long puzzled by the appearances which the liberated filaments assume. Soon after they make their escape from these attachments, they appear to become thickened at one end, and assume the appearance of large spermatozoa (*Fig.* 7 *a*). I was for some time in doubt whether this was a real bulbous thickening, or whether, as some specimens seemed to indicate, it was merely the result of a flexure of that portion of the filament, which, when *in situ*, was implanted in the outer membrane; but I am now satisfied that the latter of these is the true explanation. The base of the filament curls up (*Fig.* 7 *b c*), and produces the bulbous appearance in question. I am indebted to my friend, Mr. Dancer, for his assistance in this matter; and, after a very careful examination of the filaments, he has arrived at the same conclusion. The filament is evidently a distinct appendage, and not a mere protrusion of a part of the cell-contents through the small apertures already referred to. This is shown, both by the readiness with which they drop off, and also by the little change which they subsequently undergo, whether retained in the fluid, or dried upon the glass.

Thus far I have confined my attention to the changes which have alike modified the great bulk of the individual cells; but there are other phenomena which only affect a few of them.

Ehrenberg observed, that some "animalcules" were selected in each *Volvox*, to be the seat of changes of a different character, and that, by a continued process of division and subdivision, every one of these became converted into a young Volvox.

On examining a number of individuals, we shall find, that whilst many of them contain young fac-similes of the parent object, others exhibit no *obvious* traces of such young organisms. The latter remark especially applies to those small specimens which are the least developed. But even in these a careful examination reveals a slight enlargement of eight or nine cells, dispersed through different parts of the structure (*Fig.* 1 *d*). This enlargement goes on until each of the cells referred to, attains to a diameter four or five times greater than those by which it is surrounded (*Fig.* 2 *c*). The inner membrane also continues in close union with the cell-wall, and never assumes the stellated contour seen in the ordinary cells. In fact, the process begins whilst the germs are contained within the parent; and before the contents of any of the cells have become detached from the cell-walls (*Fig.* 3 *b*). Two new cells are soon seen to have been developed within the old one; and, by a repetition of a similar process, each of these becomes the parent of two more (*Fig.* 4 *f*). I have never yet observed one of these germs within which eight cells could be seen at once. I have no doubt that this arises from the fact, that the subsequent division has taken place in the plane of the external surface, and at right angles to the axis of vision. The next obvious development always increases the number to sixteen (*Fig.* 8), at which stage an internal cavity appears to have been formed within the germ. From this point, the multiplication of cells, by the ordinary process of cell-development, progresses (*Figs.* 9 *and* 10), until at length a condition is attained, beyond which no farther increase takes place in their number. It is at this stage, apparently, that each cell is furnished with its pair of filaments. These appendages are added before the young germ is set free by the rupture of the parent, and occasionally the young ones may be seen revolving within the old organism. This, however, is a rare occurrence, since, though their ciliæ move freely, the germs are usually stationary.

Up to this stage, each germ is still retained within a large transparent vesicle (*Figs.* 8 *a*, 9 *a*, *and* 10 *a*), apparently the relique of the cell-wall of the primary cell. Its tenuity is extreme, and not the slightest trace of structure can be detected in it; but it is invariably present. I apprehend that this is the sole bond of union with the parent *Volvox;* and that, even though the latter may be torn, unless these special vesicles be also ruptured, the young *Volvox* will not make its escape—a phenomenon which we may constantly witness. Usually, however, this membrane gives way along with, if not prior to, the laceration of the older individuals, and the ciliæ being already in action, the young ones float away, and commence their independent life.

At this time, the cells constituting the substance of the young *Volvox* are angular, fitting closely together, and having the lining membrane and endochrome in close apposition with their walls; they are in fact in the state with which we commenced our sketch. The cell-contents soon shrink away from the cell-wall, excepting at the points of contact, where the radiating threads still retain the contracting membrane in a central position, and all the other changes already described are again gone through.

In this development of germs by a process of cell division, we have merely an ordinary example of the production of a bud—a process common alike to the animal and vegetable worlds. It presents nothing like the development of an ovum, or a seed—though it is very similar in its results to the production and growth of the embryo, as it is developed from the membrane lining the embryo sac, only wanting the pollen tube and its influences, the sac being represented in the *Volvox* by the entire sphere. But though the germs produced are not true ova or seeds, may they not be endowed with a potentiality which will enable them to develop something analogous to seeds? It is consistent with what we know of other forms of or-

ganic life, to presume that the process of gemmiparous generation is not the only one through which the species is perpetuated; but that, in one form or another, germs of a different kind, capable of existing through the winter, are produced. We naturally turn to the cell-contents in the advanced stages of their development, in search of these objects, and we are immediately struck with the existence of the large granules (*Fig.* 12 *a*), of which one or two are developed in each matured cell. The production of these, which invariably exist, would appear to have been one of the objects of all the antecedent changes; and it becomes very possible that they may be the true germs, or reproductive spores. This is, of course, a fact that it would be very difficult to establish by any process of direct observation, owing to the exceeding minuteness of the objects.

When one of the cells, containing these granules, has been immersed in water for some time after the rupture of the parent, it assumes the appearance represented in *Fig.* 12; *a* are the granules in question; *b* is the brown or pinkish spot already referred to; *c* are the two filaments, which are always implanted over the contracted globular cell-contents; and *d* is the outline of the primary cell-wall.

I have already remarked, that these cell-walls are very difficult to trace in the fully developed specimens, whilst in their ordinary state; but on mounting a number of the objects for my cabinet, the fluid used being merely distilled water, I found that in a few days these cells came beautifully into view (*Fig.* 11). I have scarcely one specimen, in which a careful management of the light does not make them very conspicuous. When they are in close contact they are angular, the angles being sharp and well defined; but when the cells are apart, which is often the case, they appear more circular. The inner cell-membrane, and other cell-contents, shrink up into an irregular central mass, as in *Fig.* 11, which represents a portion of one of these specimens.

There is evidently an intercellular substance of some kind, in addition to the outer common investing membrane, which supports the filaments externally, and encloses the cells within it. The investing membrane is most probably the result of an alteration and condensation of the outer walls of some of the primary cells of the young germ. Since all the existing cells were originally developed *within* others, it is evident that the walls of the latter have either been absorbed, or they still exist in the form of thin layers, investing the cells to which they gave birth. The latter is the more probable conclusion of the two; by their development and consolidation they may have produced both the intercellular substance and the common investing integument. The existence of the former of these tissues appears to be established by specimens that have been acted upon in the way represented by *Fig.* 11.

When these objects have been mounted a few days in a solution of iodine, which, by the way, renders their ciliæ beautifully distinct, the cell-contents separate into two distinct portions. One of these is homogeneous, and of a pale green colour; the other, which comprehends the lining membrane and the granular substances, assumes a dark-brown hue.

The existence of a true internal cell or membrane lining each cell, distinct from the outer cell-wall, is obvious from an examination of young half-developed individuals, in which the cells are of comparatively large size. On subjecting one of these to gentle pressure under water, so as slightly to rupture it, the green cell-contents soon flow out, as already described. As they do so, we see that their form is capable of modification, enabling them to glide through a very narrow fissure, or to be packed together in a small space; but each one retains its pristine integrity; and, as soon as the pressure is removed, resumes its spherical form. If, on the other hand, the pressure is increased,

each of the individuals becomes ruptured, when the fluid and granular cell-contents flow out; they mingle freely with the water and with each other, but never regain their primary spherical contour.

From the foregoing outline of the principal facts presented by the *Volvox globator*, we may now proceed to consider its probable position in the kingdom of nature.

I am aware, that prior to arriving at a conclusion as to whether it is an animal or a vegetable, it will be expected that I should define what I comprehend in each of these terms. I confess myself unable to do this satisfactorily. The attempt has frequently been made by others; but none of their definitions are free from objections, or embrace all the numerous deviations from the typical forms of each kingdom. The most plausible distinction between plants and animals, is that which assigns to the former the power of assimilating inorganic mineral food; whilst the latter can only take into their systems that which is already organized. It is probable that this distinction is a valid one; but it is scarcely one of practical application as a test of special moot cases. If the digestive process involved the necessity for an internal digestive cavity with an external oral orifice, the case would be different; but there is every reason to believe, that some examples of animal organisms receive no food into internal cavities, but are endowed with a power of embracing the object on which they are about to feed, and thus absorb nutriment from the body with which they are merely in contact. The *Amœba* is still a case in point— even though we should concede the vegetability of marine and fresh-water sponges, which I am not prepared to do.

M. Agassiz and Dr. Gould, in their recently published *Principles of Zoology*, speak unhesitatingly on this point. They enumerate "distinctly limited cavities, destined for the lodgement of certain organs," as existing "in all animals without exception." They also say, that "the well-

defined and compact form of the organs lodged in these cavities, is also another peculiarity of animals. In plants, the organs for special purposes are not embodied in one mass, but are distributed over various parts of the individual." That all this is strictly true, when merely applied to the higher forms of each kingdom, cannot be denied. But surely the vegetable nature of the sponges and *Amœbæ*, closely allied as these groups are to the *Foraminifera* and other *Protozoa*, cannot be regarded as so indisputably settled as to admit of the recognition of the above generalization. If the latter objects are animals, they present all the features just quoted which these distinguished writers consider characteristic of animal life. It is only proper to add, that they entertain no doubt of the vegetable nature of sponges. They also consider "voluntary motion and sensation" as characteristic of animal life. The existence of the latter function would be difficult to prove in many undoubted animals; and on comparing the motions of the *Volvox*, of many Confervoid spores, and other vegetable organisms, with those of the ciliated germs of numerous Acrite animals, as well as those of the infusorial Animalcules, we at once perceive that the one class exhibits just as many evidences of volition as the other.

In such examples as that now under consideration, it appears to me, that we cannot safely do more than ascertain to which of the two kingdoms the object presents the greatest amount of affinity on the one hand, and the fewest discrepancies on the other. By thus weighing the various positive and negative arguments, we may arrive at an accurate conclusion, without the necessity of attempting to succeed where so many able men have previously failed. On subjecting the *Volvox* to what is apparently the only kind of test that the present state of knowledge renders practical, we are brought to the conclusion, that its true place is amongst the vegetable Algæ, rather than amongst the animal polygastric Infusoria.

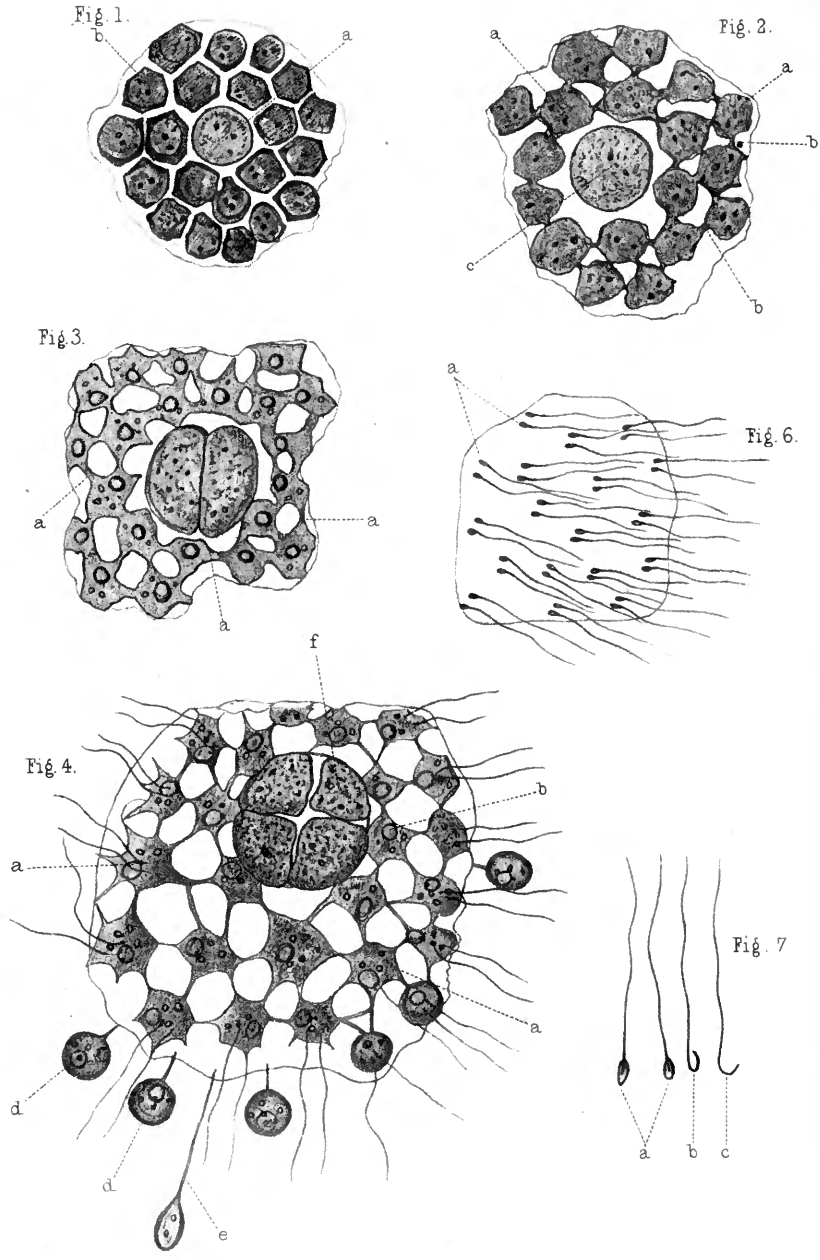
On comparing one of the cells of a young *Volvox*, prior to the shrinking of its cell-contents, with those of many of the Algæ, we find the very closest resemblance existing between them. On advancing a stage further, when the cells have become ciliated, and the organism capable of locomotion, we have a condition which is common among the zoospores of the Confervæ. In the development and temporary existence of the large green granule (*Fig.* 4 *b*), we have another point of resemblance to the Confervæ. It exists under precisely similar conditions in many of the Algæ. If we watch the development of the various species of *Coccochloris* in their earliest stages, we shall invariably detect the appearance of a single corresponding granule in the interior of each cell. It exists in the young states of most of the *Desmidiaceæ*, and is especially obvious in the species of *Cosmarium*, *Euastrum*, and *Arthrodesmus*. It is also a curious fact, that when two new segments are produced between two others of older growth (a common phenomenon amongst the *Desmidiaceæ*), each of the new portions exhibits the characteristic granule.

In some stages of its development amongst the *Desmidiaceæ*, this granule appears to contain an abundance of starch. Hence we may regard it as the analogue of the similar granules, of which a few exist in the very young cells of the *Zygenemata*, and into the composition of which starch enters largely. The number of these granules in each cell varies considerably. In *Coccochloris*, and other fresh-water Ulvaceous plants, I have never seen more than one. In the young states of the *Arthrodesmus*, *Euastrum*, and *Cosmarium*, we have invariably one, and occasionally two, in each segment. Mr. Ralfs considers each individual of these genera as consisting of but one cell, with symmetrical constrictions; and consequently we have as many granules for each cell as there are segments. I have just examined a young example of *Cosmarium margaritiferum*, which con-

tained four of these large granules, and which assumed the characteristic purple colour under the influence of iodine. In this example they were surrounded by minute dark-coloured granules in an active state of molecular motion, though themselves perfectly quiescent. In many *Desmidiaceæ*, however, I have failed to effect any change in the colour of the granule by the addition of iodine, beyond that produced upon the *Volvox* when similarly acted upon, viz., the conversion of the pale green hue into a varying tint of brown. From these circumstances, I have little doubt that the granule possesses the same nature and functions in all these known vegetable forms, and in the *Volvox*. It does not ultimately become converted into a mass of starch in many true plants, though it is in a number of instances; consequently, the absence of the violet hue in the cells of *Volvox*, when they are acted upon by iodine, neither militates against their vegetable nature, nor against my conclusion that the large green granule is identical with that seen in the cells of a young Zygenema. What may be its use I know not; but in all these cases it assumes the same form as in the *Volvox;* not existing in the first instance, but being gradually developed; and after fulfilling its office, whatever that may be, it is re-absorbed before the plant arrives at maturity.

The red speck to which Ehrenberg has assigned the functions of an organ of vision, is also known to exist in the ciliated moving zoospores of *Conferva glomerata* and *C. ciliaris*. Of the vegetable character of these spores there can be no doubt; consequently we must not only reject the physiological conclusion of the Prussian professor, but cease to regard the red speck as an indication of animal life.

The peculiar appearances presented by the shrinking of the endochrome and inner cell-membrane, as seen in *Fig.* 11, are identical with those exhibited by *Pediastrum*, *Coccochloris*, and numerous other Confervæ, when subjected to the




W. C. Williamson, Del

W Smith, Lith.

Printed by W Monkhouse, York

Fig. 5.

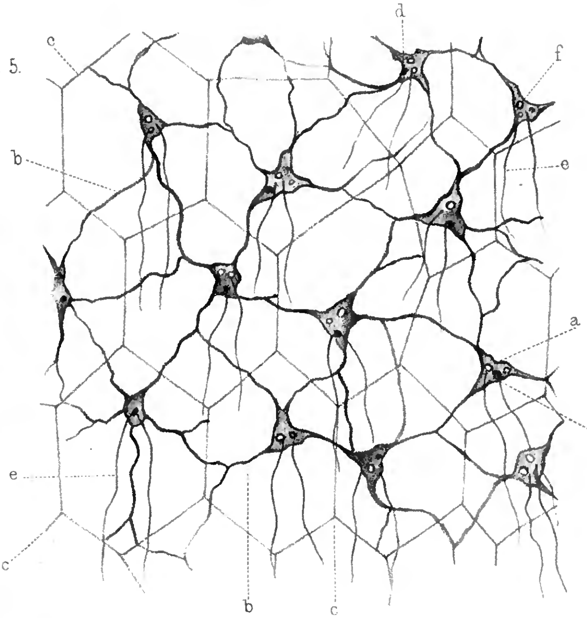



Fig. 8.

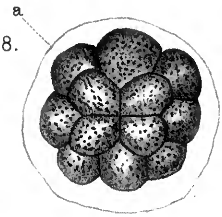



Fig. 9.

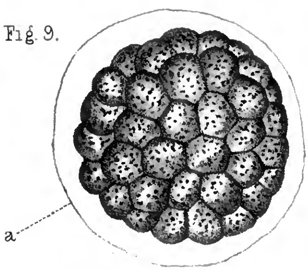



Fig. 10.

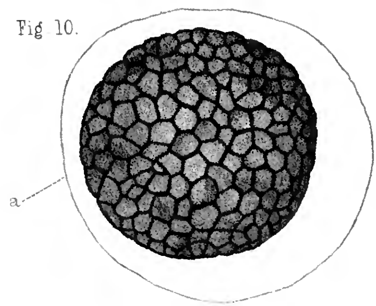



Fig. 11.

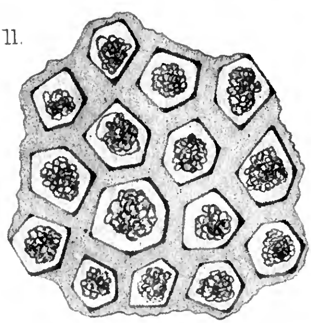
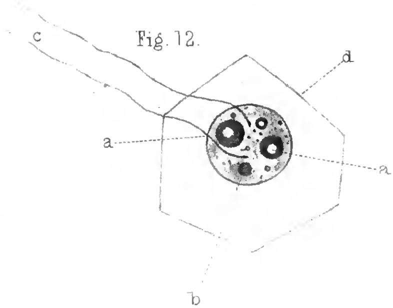


Fig. 12.




W C. Williamson, Del.

W. Smith, Lith

Printed by W Monkhouse, York

same influences. Though we have something approaching to it in the case of the white cartilage cells of animals, I have seen nothing like it amongst the undoubted infusorial forms of animal life.

In its globular form, the object approximates somewhat to the well-known *Ulva globosa.* This little parasitic plant has also a spherical contour, and consists of a saccated membrane, on the under surface of which the numerous cells are developed in a gelatinous intercellular substance. This membrane appears to be nothing more than the expanded and condensed tissues of the primary cells. I have frequently found, that when it has been ruptured, the internal cells have floated out, when its cavity has become filled with Naviculæ and other minute Diatomaceæ.

The *Volvox* exhibits a still closer affinity to the *Botridina vulgaris* of Brebisson, both in its structure and mode of growth. This latter plant, like the *Volvox,* is spherical, being primarily developed from a single independent cell; only this cell is solitary in the first instance, and not aggregate. It develops in its interior a number of other cells, of which those occupying its centre are subsequently absorbed. "The whole frond is then constituted of vesicles, closely heaped together, and inclosing in the centre, granules. *The primitive membrane, inclosing in its midst the interwoven or cellular structure, is so closely united with the peripheral stratum of vesicles, that it can in no way be separated from it.* The last development having been accomplished, the peripheral stratum of vesicles altogether loses its granules. Whether these disappear by absorption, or escape outwardly, I have never been able to perceive." Such is the description of *Botridina* given by Meneghini, as quoted by Mr. Hassal (*British Fresh-water Algæ,* vol. i. p. 320). In every point it exhibits so close an approximation to what occurs in the *Volvox,* as to leave no room to doubt that a close affinity exists between them.

In the development of the young germs of *Volvox*, we have a process closely resembling that by which the embryos of all phanerogamic plants are formed; for whilst it is seen amongst the ova of animals, it is also one of the ordinary phenomena of vegetable life.

The cells of *Volvox*, in their varying conditions, throw some light upon an interesting problem in physiological botany, since they may probably be regarded as the prototypes of a structure found in some of the higher plants. The cells entering into the composition of the hard endocarps of such fruits as the plum, and even the pear, are, as is well known, lined by successive layers of sclerogen. These layers are penetrated by tubular extensions of the central cavity, the extremities of these tubes being usually in contact with the corresponding ones of adjoining cells, as in the case of the radiating prolongations of the inner cell-membrane of *Volvox*. Now it appears probable, that we have here the same phenomena under different conditions. Dr. Carpenter inclines to the opinion, that as the inner cell-membranes become detached from the outer cell-walls in the plum-stone, they throw off successive layers of sclerogen, which occupy what would otherwise have been an intervening cavity. In the *Volvox* we have the same recession of the inner from the outer cell-wall; but we have not the cognate development of sclerogen; the intervening space being merely filled with colourless fluid. The structure of the *Volvox* cell thus appears to give support to Dr. Carpenter's explanation of those of hard endocarps, an explanation which equally applies to the development of most forms of pleurenchyma.

We thus find, that a vegetable analogue is to be found for every portion of the structure of *Volvox*, as well as of every function which those structures fulfil, so far as we can comprehend them. But on comparing it with known and undoubted animal organisms, we find that it is wanting in

many points. We have no trace of an oral orifice, or an internal digestive cavity; neither has it the compensating power of *investing* the food from which it obtains its nutriment, as we see to be the case with the *Amœbæ*, in which the possession of this power enables the outer skin to serve the purpose of an absorbent system. It can obviously obtain no nourishment, excepting what exists in a state of solution in the water—the latter being apparently absorbed by a process of endosmose. This, as already pointed out, is the common condition of the early forms of vegetable life.

From this close approximation to vegetable types, and the absence of every thing that appears peculiarly characteristic of animal organisms, I am led to conclude that the *Volvox* is a true plant. If this conclusion be a correct one, it becomes increasingly probable that *Euglena*, and a number of allied ciliated objects, regarded by Ehrenberg and others as polygastric animalcules, will also be found to belong to the same division of the organic world.

## XXII.—*On the Structure and Affinities of the Plants hitherto known as Sternbergiæ.* *By* W. C. Williamson, *Professor of Natural History in Owens College, Manchester.*

Read September 25, 1851.

If we except a vague representation, given by Count Sternberg, the first published notice of the plants now known as *Sternbergiæ* appeared in the *Antediluvian Phytology* of Artis,* who assigned to some English forms the name of Sternbergia approximata. They were subsequently examined by M. Brongniart, and included in his *Prodrome;*† but at that early period this distinguished writer had not enjoyed the more extended opportunities of studying their character with which he has since been favoured. He then regarded the concentric superficial rings as probably marking the insertion of amplexicaul leaves; and concludes that, "ces anneaux d'insertion, très-rapprochés, sont fort analogues à ceux qu'on voit sur les tiges des *Yucca*, de *l'Aletris fragrans* et de plusieurs Liliacées arborescentes." After pointing out, also, their resemblance to the stems of *Pandanus*, he observes, "on peut donc présumer que, lors de la formation des terrains houillers, il existoit un très-petit nombre de plantes monocotylédones arborescentes, à tiges analogues surtout à celles des *Yucca* et des *Aletris*, portant des feuilles fort semblables aussi à celles des plantes de ces genres." But with that caution which has ever charac-

* London, 1825, p. 8.

† *Prodrome d'une Histoire des Végétaux Fossiles.* Par M. Adolphe Brongniart, Paris, 1828.

terised his labours, M. Brongniart adds, "mais cependant, comme l'analogie de ces tiges, de ces feuilles et de ces fruits avec la famille des Liliacées offre encore quelques doutes, d'autres familles monocotylédones présentant à peu près les mêmes caractères, nous préférons laisser ces portions de végétaux parmi les monocotylédones de famille douteuse." * He then enumerates three species: S. transversa of Artis, S. approximata, and S. distans—but, of course, without appending to them any descriptions.

I am not acquainted with any further notice of these plants, until the publication of excellent drawings of specimens from the English coal-measures, by Lindley and Hutton, in the *Fossil Flora of Great Britain.*† Without pretending to add any thing to the existing knowledge respecting the true affinities of these objects, the authors point out the fact, that the specimens "are covered with fine coal, which either adheres in the form of an even thick glossy integument, or adheres in a powdery state to the surface of the stem."

The examination of specimens from the coal formation of Nova Scotia, by Mr. J. W. Dawson and Mr. J. S. Dawes, in 1846, threw some additional light upon the Sternbergiæ; though without materially removing the obscurity which invested this curious genus. Mr. Dawson remarks, that his specimens "are in the state of stony casts, always invested with a thin bark or outer coating of lignite, whose outer surface is smooth, and without transverse wrinkles. The inner surface of the coating of lignite has longitudinal ridges, which adhere strongly to the surface of the transversely striated cast, and leave marks or small furrows when removed." Mr. Dawson also observes, "transversely ridged stems, of a character very different from the above, are, however, occasionally found in the carboniferous beds

* *Prodrome*, p. 124.

† *Fossil Flora of Great Britain.* Tab. 224 and 225, 1835.

of this province. These are stony casts, having irregular and often large transverse markings, and enclosed in a thick coat of lignite or fossil wood. In two specimens of the latter kind, transverse sections of the portion with structure show cellular tissue, apparently with medullary rays, and much resembling the wood of *Coniferæ.* The fossils last mentioned are probably, as suggested by Mr. Dawes with reference to the British species of Sternbergiæ, casts of the pith of trees. It appears evident, however, that the first-mentioned species (named, I believe, Artisia approximata in Sir C. Lyell's list), was a plant having a very large pith, and a comparatively thin woody envelope—in short, a gigantic rush-like plant, perhaps leafless, and nearly cylindrical, like some modern species of Juncus." *

At the meeting of the Geological Society which succeeded the reading of Mr. Dawson's memoir, Mr. J. S. Dawes communicated his "Observations upon Sternbergiæ." He "does not agree in the conclusions of Mr. Dawson, that herbaceous endogens may sometimes have produced the columnar forms, usually referred to the pith of coniferous and other large trees. He thinks, that although the appearance in question may indicate that the fossil is not that of a true dicotyledon, it must still have been the interior cellular portion of an arborescent plant, like Lepidodendron; the supposed bark being the vascular system or sheath surrounding the pith, which has adhered during decay to the medullary column, and sometimes been changed into coal." †

The investigations of M. Corda at length revealed the fact, that some of the objects previously regarded as Sternbergiæ and Artisiæ, were but the medullary cylinders of a genus allied to Lepidodendron, which he terms Lomatophlois; and which, as well as the Pachyphleus of Goeppert,

* *Proceedings of the Geol. Society,* London, No. 6, Jan. 1846.

† *Ibid.*, No. 6, p. 139, 1846.

M. Brongniart unites with the genus Lepidophloios of Sternberg. Receiving the above determination of M. Corda as an established truth, and having enjoyed new opportunities of studying the subject, M. Brongniart has, in a recent publication,* modified his former opinions respecting Sternbergiæ, as published in his *Prodrome*.

He does not appear to have been acquainted with the observation of Mr. Dawson, that in some cases Sternbergiæ occurred in connection with wood of a coniferous type; nevertheless his acute mind led him to conclude, that some forms of Sternbergia had no affinity to Lepidophloios. Speaking of the piths of this latter genus, he observes, "Ces axes peuvent avoir quelquefois été confondus avec les vrais Artisia; et je crois que ceux figurés par M. Sternberg † sont dans ce cas; mais je doute qu'il en soit toujours ainsi, et je pense qu'il y a des tiges désignées sous ce nom encore mal connues, qui sont étrangères aux Lepidophloios; celles des mines d'Angleterre me paraissent surtout dans ce cas." ‡ Whilst M. Brongniart thus separates the English Sternbergiæ from Lepidophloios, he refrains from giving any definite opinion of his own respecting them; but from the re-introduction of some of his previous remarks amongst his observations on the Liliaceæ, he would appear to retain a vague impression as to the probable affinity of the English Sternbergiæ with some of the monocotyledonous plants. With his accustomed caution, however, he adds, that the characters which appear to ally them with Pandanus, for example, "sont assez vagues et dénaturés à laisser des doutes sur la nature de ces végétaux."

Such, as far as I am aware, is the amount of what had been accomplished by previous observers, when the examina-

* *Tableaux des Genres des Végétaux Fossiles. Dictionnaire Universel d'Histoire Naturelle*, Paris, 1849.

† *Fl. des Vorw.*, 2, t. 53, f. 1-6.

‡ *Tableaux des Genres*, &c., p. 44.

tion of some specimens which fortunately fell into my hands, enabled me to clear up many points which were hitherto obscure. Fifteen years ago, a very large and fine specimen of Sternbergia approximata came into the possession of the late Dr. Charles Phillips of Manchester, and was by him presented to the museum of the Manchester Natural History Society. In this specimen, the Sternbergia was invested by a thick ligneous covering, as in the case of Mr. Dawson's examples from Nova Scotia; but of what kind of tissue this ligneous zone consisted I could never ascertain, owing to the loss of the specimen, which, becoming decomposed, was, I understand, thrown away. After the death of Dr. Phillips, his private collection of fossils passed into the hands of my friend, John Bury, Esq. of Scarborough. On examining the collection a few weeks ago, I found in it the fine fragment which I have represented by fig. 10, and for the loan of which I am indebted to Mr. Bury; it is, doubtless, a portion of the same plant as the large specimen already referred to. The minute examination of its structure clearly established the coniferous character of the ligneous cylinder, but it threw very little light upon the real nature of the Sternbergia which it invested.

My own collection contained a small fragment of fossil wood from Coalbrookdale, enclosed in an ironstone nodule, for which I was many years ago indebted to the able historian of that interesting field, Mr. Prestwich. On subjecting this specimen to a careful examination, I found that it also contained a form of Sternbergia. But what was of the highest importance, it exhibited in exquisite perfection all the other tissues of the plant, from the epiphlœum to the medulla; thus enabling me, not only to establish the character of the plant, but also to settle the real nature of the so-called Sternbergia. From the study of this latter specimen, along with many others, I have come to the conclusion, that Sternbergia approximata belonged to a coniferous plant

allied to Araucaria, exhibiting the structure and arrangement of woody fibre and medullary rays, which, according to M. Brongniart, characterise Endlicher's genus Dadoxylon. But its pith, instead of being like those of the living Araucariæ, belonged to the curious type termed discoid or disciform, resembling those of the recent walnut and white jasmine; whilst the so-called Sternbergiæ were neither the remains of monocotyledonous stems, nor yet the piths of exogenous plants, but *mere inorganic casts of central cavities existing within the true piths*, which cavities had, under favourable circumstances, become filled with some inorganic material. Though subsequent chemical changes have usually caused either the total disappearance of the vegetable elements, or their conversion into a thin film of carbonaceous matter, these casts of the moniliform cavities have been permanently preserved.

Fig. 1 represents a portion of the Coalbrookdale fossil split longitudinally, and revealing the different tissues. Even with the naked eye we can distinguish an external bark (*a* and *b*), a middle woody layer (*c*), and an internal medulla (*g*), within which latter tissue is the central cylinder, representing the so-called Sternbergia (*h*). In order to leave no doubt about the true nature of each of these structures, I have given representations of sections made both horizontally and vertically. Fig. 1 is enlarged to nearly double the size of the original. Fig. 2, which represents the extremity of the fragment where it has been broken across, is of the natural size. We see in it the same concentric zones of bark (*a* and *b*), wood (*c*), and medulla (*g*), surrounding the central cylinder (*h*), as in fig. 1. Fig. 3 represents a *transverse* section of the bark, including a few fibres of the woody layer, which show that we have reached the innermost portion of the former structure. The external half (3 *a*) consists of large irregularly formed cells of unequal sizes, and appears to represent the

epiphlœum or corky layer, and the mesophlœum or middle layer of the bark. In fig. 4, which represents a *vertical* section of the same tissues, only enlarged to about twice the size of fig. 3, the cells of the epiphlœum and mesophlœum (4 *a*) are still more regular and distinct. In this section they are usually square, and arranged in interrupted vertical rows, which sometimes tend a little outwards towards the exterior of the bark. In the horizontal section their square contour is less obvious; they exhibit more of the corrugated aspect seen in the dried bark of a recent Araucaria. Fig. 3 *b*, is obviously the endophlœum or inner bark, which is of considerable thickness. Its true character is better seen in the vertical section, fig. 4 *b*, where its distinctness from the mesophlœum is very obvious. As in the recent Araucaria, it consists of some cellular tissue, along with a vast number of the elongated ducts which are so conspicuous in the inner bark of the recent plants, and which, I presume, are the laticiferous vessels. In the horizontal section (fig. 3 *b*), numbers of these vessels are faintly seen, ascending from below in an oblique manner. In the vertical section, the cells of the epiphlœum have an average diameter of about the $\frac{1}{500}$ of an inch. Those of the mesophlœum are smaller, and a little more elongated vertically. The diameter of the laticiferous vessels is about $\frac{1}{1200}$ of an inch.

Nothing can be more distinct than the line of demarcation between these tissues and the woody zone (*c*). In the horizontal section (fig. 5), this zone exhibits the ordinary aspect presented by coniferous wood from the coal measures.

It consists wholly of pleurenchyma or woody fibre, arranged in radiating lines (fig. 5 *c*), and separated at intervals by intervening medullary rays (fig. 5 *d*). There are no concentric lines of growth, such as are seen in recent Araucariæ and other coniferæ. This feature of many of the fossil woods of the coal measures has been

already noticed by M. Brongniart. He characterises one of his groups of coniferous woods by "L'uniformité de densité du tissu, d'où resulte l'absence de couches annuelles distinctes, caractère qui appartient surtout à des bois des terrains anciens, évidemment étrangers aux vrais *Pinus* dont il n'y a aucune trace dans ces formations."—(*Tableaux des Genres de Végétaux Fossiles*, p. 76.)

In fig. 6 is represented the aspect of this tissue in a vertical section, cut parallel with the medullary rays, which are seen crossing the section at 6 *d*. The fibres have an average diameter of the $\frac{1}{800}$ of an inch, and even under a low magnifying power their walls are seen to be covered with minute reticulations. On applying an object glass having a magnifying power of about 280 linear, we obtain the beautiful appearance represented by the sketch fig. 7, in which is delineated a small portion of two of these fibres as they appear in the above section.

The reticulations are obviously caused by the apposition of alternating rows of the disks characterising coniferous pleurenchyma. The language which M. Brongniart employs in describing the genus Dadoxylon is strictly applicable to them: 'Ces especes ont, en éffet, la plupart des caractères essentiels du bois des Araucaria, c'est-à-dire les ponctuations des fibres ligneuses disposées en plusieurs series, alternantes entre elles, et prenant par pression la forme d'aréoles hexagonales.'—(*Tableaux des Genres, &c.*, p. 76.) Their appearance in my sections cannot be better described than by the above sentence. None of the specimens which I have examined exhibit any traces of the central punctuation, which, in the existing coniferæ, characterises each areola. In the recent Araucariæ it is generally distinct; in A. Brasiliensis it is very curious, being of a crucial form, but the two elongated and traversing limbs of the cross are not on the same plane, being apparently on opposite sides of the intervening lenticular disk. Either this

central punctuation has had no existence in the fossil species, or it has disappeared in the process of fossilization: I am unable to say which. It is certain, that in none of the numerous sections of coniferous wood from the coal measures which have come under my notice, have I been able to satisfy myself of its existence.

In the section under consideration, the usual number of vertical rows of disks on each fibre is two, but occasionally there are three; the average diameter of each disk being about the $\frac{1}{2000}$ of an inch. The fibres are readily detached from each other.

On making a section parallel with the bark, or at right angles to the medullary rays, we have the structure delineated in fig. 8, and still more highly magnified in fig. 9. No disks exist on this surface of the pleurenchyma; but along the line of junction of each two contiguous fibres, there are a number of transverse bars (fig. 8 *a* and 9 *a*). These appear to be the lines of division between different disks; since both in size and position they correspond exactly with the areolæ. Professor Balfour describes these disks as "concave depressions on the outsides of the walls of contiguous tubes, which are closely applied to each other, forming lenticular cavities between the vessels, like two watch-glasses in apposition."* The appearances presented by fig. 9, seem to corroborate this interpretation.

At fig. 8 *d*, are the intersected medullary rays, consisting of single layers of superimposed cells. This characteristic identifies the specimen with the first of M. Brongniart's subdivisions of fossil coniferous woods. The lateral aspect of the medullary rays is represented in fig. 6 *d;* where the section has been ground very thin, these rays are seen to consist of the ordinary form of mural tissue.

The structure of the entire woody zone bears a very close resemblance to that of the Dadoxylon Brandlingi,

* *Manual of Botany*, London, 1849.

*Brong.*,* with which plant the specimen exhibits other points of affinity, to be noticed immediately.

Within this zone of glandular fibre or pleurenchyma is a thin layer of spiral vessels, constituting a true medullary sheath (figs. 5 *e* and 6 *e*). The diameter of the vessels is rather less than that of the fibre, but their spiral character is sufficiently obvious to establish their nature. At the inner surface of the medullary sheath there are a few vertically elongated cells (fig. 6 *f*), and within these is the true medulla or pith, occupying the position marked *g* in the figures 1, 2, 5, and 6. The most striking feature of the last structure, is the perfect preservation of the individual cells. No recent plant could exhibit the tissue in a more beautiful condition. The sections of the cells are usually hexagonal; in fig. 6 they are a little compressed vertically, and exhibit a marked disposition to arrange themselves in perpendicular series. In the horizontal section they appear as perfect hexagons, having an average diameter of $\frac{1}{400}$ of an inch, many of them being considerably larger.

So far, the structure of this interesting fragment is that of an ordinary Araucarian plant, the chief exception arising from the non-existence of the annual concentric layers in the woody zone; but we now reach a point, where, so far as I am familiar with the structure of the recent coniferæ, this resemblance ceases. *Within* the true medulla, we find the curious cylinder *h* in figs. 1 and 2. It is solid throughout, and quite structureless; consisting merely of granules of one of the oxides of iron and a little iron pyrites, the result of chemical infiltration and deposition. *On its exterior, where it has come in contact with the inner surface of the hollow medulla, it exhibits the transverse markings of a Sternbergia.* Here we obtain the solution of a problem which has so long perplexed the students of fossil phytology. The plant has had a hollow pith, and Sternbergia approximata is merely the

* Pinites Brandlingi, *Witham. Internal Structure of Fossil Vegetables*, Edinburgh, 1833.

cast of this hollow internal cavity, which has been filled with various inorganic materials. In some cases the formation of the internal mould has been the result of chemical infiltration; but it more commonly consists of sandstone, which has entered mechanically at the patent extremities of the fragment. The transverse lines and ridges with which its surface is always sculptured, prove that the interior of the medullary cavity has not been smooth, but has exhibited a modification of the curious medullary type termed discoid or disciform.

In a few recent trees, belonging to the widely separated orders of Jasmineaceæ and Juglandaceæ, the pith, which is at first solid and homogeneous, soon undergoes a change. Portions of it become absorbed, whilst the remainder exists as a thin layer, lining the medullary sheath, from which layer these discoid laminæ are extended across the medullary cavity (fig. 12). In the first instance these laminæ are thick and in close contact; but by the continued absorption of the medullary matter, their thickness is diminished, and the intervening spaces proportionately enlarged. It is possible, also, that the growth and consequent elongation of the woody zone may cause some further divergence of these laminæ; but if so, the change which they undergo from this source is very small, since their average distances in small twigs and in thick stems is very similar. This type of pith does not appear to characterise any particular groups of recent plants. It is very obvious in the common white Jasmine (Jasminum officinale), but does not occur in the yellow species (Jasminum humile and revolutum). It exists in the Juglans regia, or common walnut, as well as in some species of Carya or hickory; but is absent in others. In all cases where it occurs, the pith is, in the first instance, solid; its subsequent laminated aspect being the result of a secondary process of absorption. The laminæ are thickened at the base (12 *c*); so that in a vertical section, the intervening spaces present a concave peripheral

outline. Many of the laminæ subdivide on one side into two—a small incomplete space, not extending across the entire medullary canal, being thus intercalated.

Now, all this is precisely what has occurred in the so-called Sternbergiæ, only the process of absorption has been carried a step further than in any of the recent examples already quoted. In addition to the formation of the thin transverse disks, the centre of each disk has usually been absorbed, leaving merely flattened rings of medullary tissue, united to the rest of the pith by their thickened peripheries. Thus, instead of the intermediate cavities being isolated, they have apparently been connected together by a central fistular passage. The appearance presented by a vertical section of the pith, when recent, would apparently correspond with fig. 11, which is a restored sketch of what I suppose this structure to have been. The gradual thickening of the exteriors of these rings would give a concave outline to each cavity when intersected vertically, which would of course be represented in the *cast* by the rounded outline seen in the peripheries of what in Sternbergia were thought to be horizontal plates; and the divergence of many of the laminæ accounts for the apparent intercalation of incomplete disks, in the fossil, between those which can be traced round its entire circumference. Examples of this feature are seen in fig. 10. Well might botanists wonder at the anomaly of vegetable structures assuming this strange arrangement of "horizontal plates, held together by some connection in the axis of the stem," and add, "a most extraordinary appearance, to which we know of no parallel."—(*Lindley and Hutton's Fossil Flora of Great Britain*, vol. iii., p. 188). The anomaly is now explained; this wonderful arrangement of vegetable tissues has no real existence.

In the Coalbrookdale specimen (fig. 1), the transverse ridges are exceedingly numerous, and in very close contact. In this respect they differ a little from the ordinary

forms of Sternbergia, in which they are much thicker and less numerous; implying, of course, an inverse arrangement of the medullary laminæ. These differences are but the parallels of what has been already stated to exist in the recent plants, and may either depend upon the age of the plant, or indicate a difference of species; probably both.

Fig. 10 represents the beautiful specimen from the collection of the late Dr. Charles Phillips, for which I am indebted to John Bury, Esq. of Scarborough. The drawing is two-thirds the size of the original. We have here (fig. 10 *a*) the more ordinary form assumed by the English Sternbergia approximata, surrounded by a cylinder of ligneous tissue, presenting the same structure as that already described; only, instead of the woody zone being little more than the $\frac{1}{10}$ of an inch in thickness, it here reaches half an inch. The narrow space between this ligneous zone and the so-called Sternbergia, is filled up with pulverulent carbonaceous matter, which is obviously the mineralized residue of the true medullary tissue; the structure of which, in this, as in almost every other instance hitherto discovered, has been lost. The exterior of the ligneous zone is smooth and carbonaceous—being, in fact, the outer surface of the decorticated wood. In all these points it appears to resemble the specimens found by Mr. Dawson in Nova Scotia.

In the exterior of the wood of fig. 1, the fibres of pleurenchyma are very distinct, even when viewed through a low magnifier; and, what is interesting, there are numerous points where the fibres diverge a little, meeting again lower down. Vertical sections of these points show them to be abortive buds and branches. In the interior of each is an extension of the central medullary tissue, which, of course, has not as yet assumed the discoid arrangement. This process is reserved for a later stage of its development.

At the exterior of fig. 10, the carbonaceous matter obscures the fibres; but in the fractured surfaces a very low

magnifier brings both them and the lenticular medullary rays distinctly into view.

The chief question that remains to be considered, is the relation in which the subjects of this memoir stand to the innumerable fragments of coniferous wood, found in the coal measures of every part of the globe. I have already pointed out the very close resemblance, if not identity, between the structures just described, and the Dadoxylon Brandlingi (Pinites Brandlingi, *Witham*), or fossil tree from Wideopen, near Newcastle. Mr. Witham describes all the species of his genus Pinites as characterised by "a medullary axis of very large size." We have already seen that this constitutes a striking feature of the Sternbergiæ; indeed, in the case of fig. 10, the large size of the medullary cavity, compared with the small thickness of the woody zone, is very remarkable. It is well known, that when once the pith is invested by a zone of woody tissue, it undergoes no further increase of size; so that the diameter of the pith, at least, reveals to us the thickness of the extremity of the growing branch. In the case of fig. 10, the diameter of the pith has not been much less than two inches. We may infer from this, that the habit of the plant has not been that of Pinus larix, or Cedrus Deodara, giving off long slender pendent branches; but rather that of Araucaria excelsa and imbricata, or Pinus Pallassianus. The branches appear to have been *straight*, as in the two former examples, and, with thick succulent growing extremities, presenting exaggerated examples of what we find in the latter tree. In young plants all these features would be much more marked than in the terminal twigs of larger trees, which may account for the differences between the various specimens.

It is only by admitting this supposition, at least in the case of the young trees, that the very large size of the pith can be explained. It is well known that, after the medulla becomes invested by a woody zone, it undergoes no further

enlargement. It is a moot point whether or not its diameter is at all affected by the subsequent growth of the tree; but, if it is so affected, the change tends to a diminution of diameter, rather than to an enlargement. But, be this as it may, it is certain that Sternbergia has been characterised by a remarkably large medullary cavity, and that a similar feature is exhibited by the examples of Mr. Witham's genus Pinites; neither is there any material difference in the structure and arrangement of their pleurenchyma, though they present slight diversities in the form of their medullary rays. These points of affinity render it probable, either that they have contained a structure like Sternbergia, which has been overlooked, or that the whole of the cellular tissue has disappeared prior to fossilization.

In the Lancashire coal-field, as elsewhere, fragments of coniferous wood are not unfrequent. Mr. Dawson considers that the fragments of mineral charcoal, which are of common occurrence, may be of the same nature. Such, however, is not the case. The structure of the fibres or ducts (whichever they may be), in the latter substance, are peculiar and distinct, as has been already pointed out by Dr. J. Hooker. The latter observer looks upon the structure as allied to the tissues of Cycadeæ. They appear to me to exhibit a more marked resemblance to the perforated ducts of Tmesipteris.—(*See Brongniart's Végétaux Fossiles*, Vol. ii. Tab. 11, fig. 6 *b*). Be this as it may, it is certain that they are very different from coniferous pleurenchyma. The latter tissue, however, is sufficiently abundant. I possess one remarkably fine specimen of fossil wood from near Wigan, in which a thick branch passes quite through the wood, like a knot in a piece of common deal. In the centre of the branch there is a pith of considerable size, though nothing approaching to the dimensions of that of fig. 10. I believe it to be very probable that this specimen belongs to the same genus of coniferæ as the Sternbergia, if not even to the same

species, which is doubtful; it affords demonstrative evidence that the trees have been branched in the same way as the ordinary gymnospermous exogens.

The collection of Mr. Binney contains some fine portions of large coniferous trees. Some of these have had remarkably thick piths, besides which their pleurenchyma exhibits the same structure as that which I have delineated in fig. 7. It appears very probable, that not only these have belonged to the same plant, but that the entire group of coniferæ from the coal-measures, constituting M. Brongniart's genus Dadoxylon, are the ligneous portions of the trees of the piths of which Sternbergia approximata represents internal casts.

With the foliage of these trees we are as yet unacquainted; we can scarcely regard it probable that all traces of it have disappeared, if we bear in mind the comparative frequency with which portions of stems and branches are met with. The foliage of true coniferous plants is of frequent occurrence in the oolitic shales and sandstones of the Yorkshire coast; consequently, there is no reason why they should not also have been preserved in the similar strata of the carboniferous epoch. It is possible that the foleaceous appendages of Dadoxylon may be represented by some of the well-known plants of the coal-measures, which have hitherto been confounded with Lepidodendron. The beautiful coniferous plants from Yorkshire, of which Lycopodites Williamsonis is the great type, were for many years regarded even by Brongniart as belonging to the Lycopodeaceæ. There is therefore nothing improbable in the supposition, that phytologists may have fallen into a similar mistake respecting those of the coal-measures. The young shoots of many coniferæ are covered with elongated markings closely resembling those of a Lepidodendron; consequently we might expect that the external bark of the young twigs of Dadoxylon would be sculptured in the same way. If so,

nothing is more probable than that they should at first be confounded with one another, especially since the carbonized condition in which these terminal twigs occur, has led to the disappearance of all internal structure. But for this disappearance nothing would have been easier than the identification of the missing organisms, owing to thick spiral vessels of the one group of plants being so very different from the glandular pleurenchyma of the other.

The existence of a plant with a discoid pith at so early a period as the carboniferous epoch, is a fact of considerable interest to the phytologist. It affords new evidence of the unity of the great plan upon which the Creator has acted from the beginning of time. By varying the combinations of a few elementary tissues, He has produced all the marvellous forms of vegetable life which have constituted the Flora of both the present and the past. It is very questionable whether the modern era has witnessed the creation of a single new tissue, or even the disappearance of a pre-existing one. To this, Sternbergia is no exception. That a peculiar form of pith, which is now found only in a few Jasmineaceæ from India, and in the widely different Juglandaceæ of Cashmere and North America, should find a prototype amongst the Coniferæ of the primeval world, is a curious circumstance.

It is manifest that the genus Sternbergia, as adopted by Artis and his successors, exists no longer. Some of its forms having been already assimilated to Lepidophloios, by M. Corda; and the most characteristic of those which were left being now identified with Dadoxylon—there remains little or nothing to be represented by the name. At all events, Sternbergia approximata may henceforth stand as Dadoxylon approximatum; whether or not all the species of Dadoxylon have possessed this form of pith, is immaterial, since it will not affect the integrity of the genus as defined by M. Brongniart, any more than the similar want of it

Fig. 3

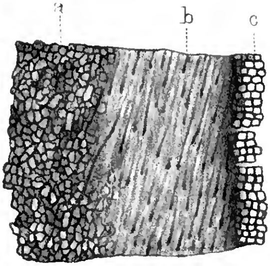



Fig 4

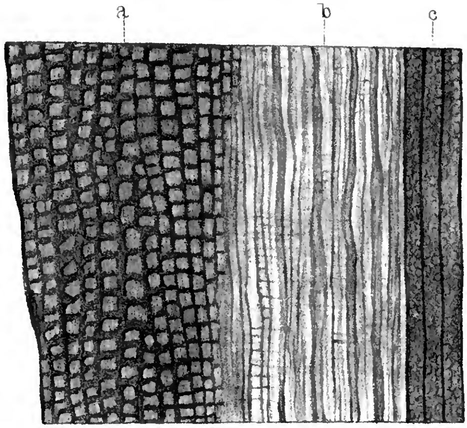



Fig. 1.

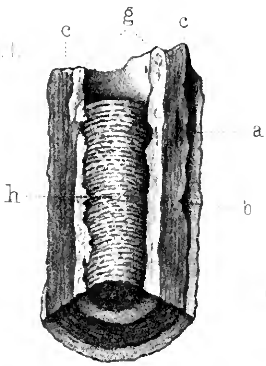



Fig. 5.

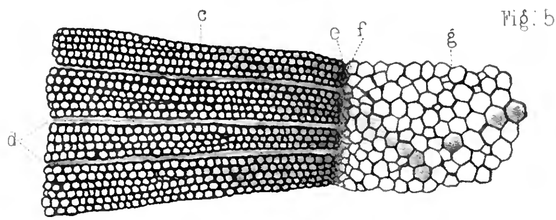



Fig. 2

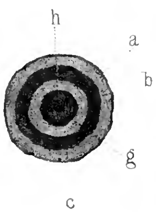
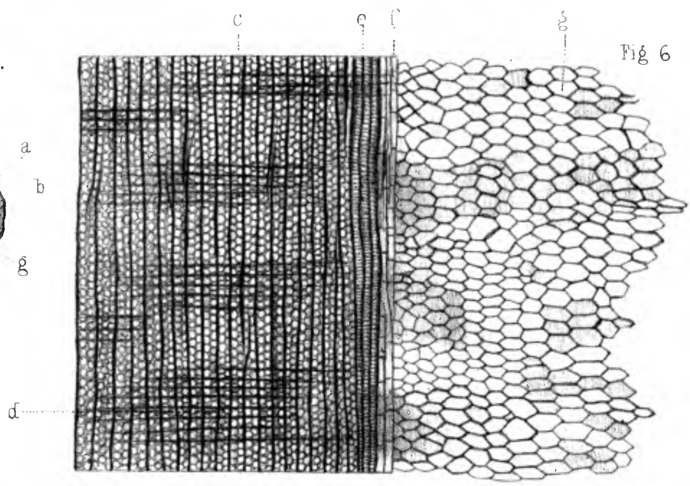



Fig 6




A. . Williamson, Delt W Smith, Lith

Fig. 7.

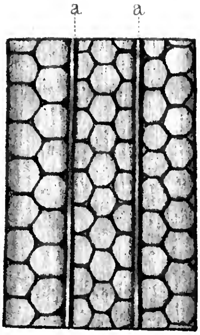



Fig. 10.

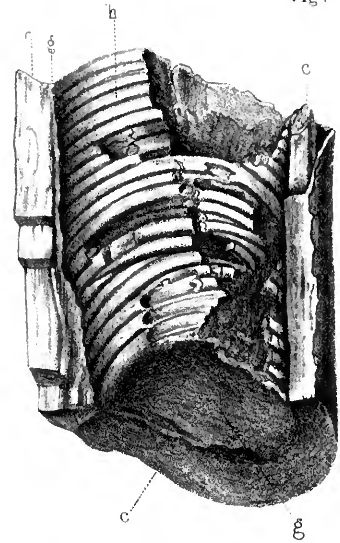



Fig. 9.

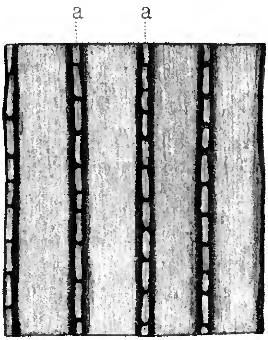



Fig 11.

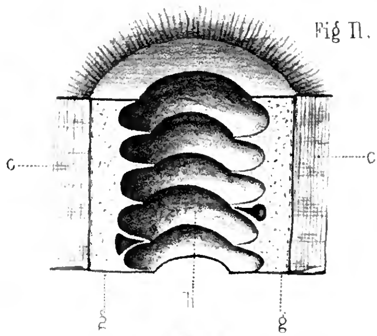



Fig. 8.

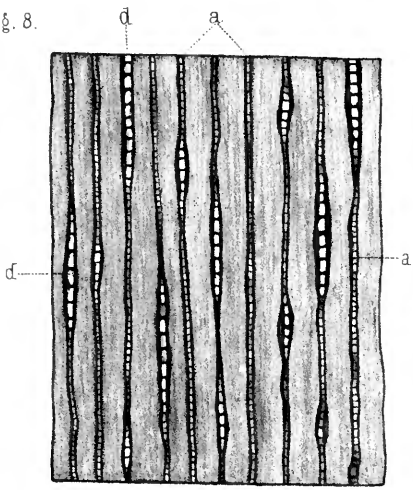
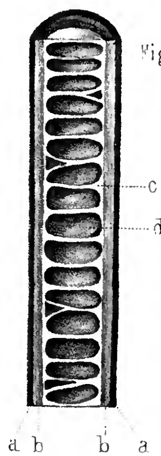



Fig. 12.




43

W C Williamson, Delt. Printed by W. Monkhouse, York W Smith, Lith

separates the yellow from the white jasmine, or some species of Carya in which it exists, from the others in which it does not. It has never been regarded as constituting a generic distinction.

Subsequently to the penning of the preceding observations, I have been favoured by G. W. Ormerod with permission to examine a specimen of Sternbergia obtained from the celebrated quarry at Peel, in Lancashire. I find in this interesting fragment demonstrative evidence of the accuracy of my previous determination. The specimen is partly covered with the usual thin film of carbonaceous matter, *in which the cellular structure is beautifully preserved;* the cells, which exhibit a very strong disposition to be arranged in vertical lines, have also left a definite impression upon *the exterior* of the Sternbergia, which consists of clay ironstone. Horizontal laminæ of brown carbonaceous matter are prolonged inwards from the smooth investing layer, and separate the contiguous disks. In these laminæ, also, the cellular structure is beautifully defined. In the disks intervening between the cellular laminæ, there is no trace of structure whatever. They wholly consist of inorganic clay ironstone. This specimen appears also to support my conclusion, that the centre of each horizontal lamina has usually been absorbed; but I cannot decide positively whether this has actually been the case, or whether the clay has been forced in at one extremity by an external pressure, which has been sufficient to break through the delicate layers of piths, and thus connect the disk—like portions of the cast at their centres. One part of the specimen exhibits a very different external aspect from the remainder, showing how very much influence mere pressure has had in modifying the external surfaces of the so-called Sternbergiæ.

INDEX TO THE PLATES OF THE ABOVE PAPER.

Fig. 1. Fragment of wood from Coalbrookdale, fractured longitudinally; *a* and *b*, bark; *c*, woody zone; *g*, medulla; *h*, cast of hollow pith, or Sternbergia.

2. Transverse section of the same fragment. The same letters are employed as in fig. 1.

3. Transverse section of the bark; *a*, Epiphlœum and Mesophlœum; *b*, Endophlœum; *c*, Pleurenchyma, or woody fibre.

4. Vertical section of the bark; *a*, Epiphlœum and Mesophlœum; *b*, Endophlœum; *c*, Pleurenchyma.

5. Transverse section of the woody zone and pith; *c*, Pleurenchyma; *d*, medullary rays; *g*, medulla.

6. Vertical section of the same portions as fig. 5; *c*, Pleurenchyma; *d*, medullary rays; *g*, medulla.

7. Fibres of Pleurenchyma from fig. 6, still more highly magnified, and exhibiting the hexagonal disks.

8. Vertical section of the Pleurenchyma, parallel with the bark, and at right angles to the medullary rays; *a*, transverse bars marking the positions of the disks; *d*, intersected medullary rays.

9. Small portion of fig. 8 still more highly magnified.

10. Specimen of Sternbergia from the Lancashire coal field; *c*, woody zone; *g*, pulverulent carbonaceous matter, representing the medulla; *h*, cast of medulla, or Sternbergia approximata.

11. Vertical section of restored medulla of Dadoxylon (Sternbergia) approximatum; *c*, woody zone; *g*, medulla; *h*, cast of medullary cavity.

12 Vertical section of a young branch of Carya angustifolia, or hickory; *a*, bark; *b*, wood; *c*, pith; *d*, hollow cavities.

# INDEX TO VOL. IX.


PAGE.

BATEMAN, JOHN FRED., Report on the Fall of Rain........................ 1

BINNEY, E. W., Description of a Mineral Vein in the Lancashire Coal Field near Skelmersdale........................................................ 115

——— A description of some Supposed Meteorites found in seams of Coal ........................................................................ 306

——— Remarks on a Vein of Lead found in the Carboniferous Strata in Derbyshire.................................................. 125

Bridges, Tubular Girder, on the Security and limit of Strength of, constructed of Wrought Iron ........................................... 179

CALVERT, F. CRACE, Memoir on the Oxides and Nitrates of Lead...... 130

CLARE, PETER, Report on the Fall of Rain................................. 1

Coal, Chemical Changes attending the Formation of.................... 250

Coal Field, Mineral Vein in the Lancashire, near Skelmersdale........ 115

Dew, on the Formation of.................................................. 46

Engine, Locomotive, Experimental Enquiries into the relative powers of, and the resistance of Railway Gradients............................ 149

Equations, on Impossible and certain other Surd......................... 207

——— on Impossible ........................................................ 236

FAIRBAIRN, WILLIAM, F.R.S., Experimental Enquiry into the Relative Powers of the Locomotive Engine, and the resistance of Railway Gradients.................................................................. 149

——— On the Security and limit of Strength of Tubular Girder Bridges constructed of Wrought Iron ................................... 179

Farming, Faults in ........................................................ 93

FINLAY, ROBERT, Professor, on Impossible Equations .................. 236

Gas, on the Composition of, produced by the joint Distillation of Tar and Water at a high temperature ......................................... 243

PAGE

Gas-making, on the Chemical Changes attending the Formation of Coal, and the relation of these changes to the philosophy of Gas-making ........ 250
Gaseous Mixtures, on the Analysis of ........ 297
GOODMAN, JOHN, Researches into the Identity of the Existences or Forces of Heat, Light, &c. ........ 80
Gradients, Railway, Experimental Enquiry into the Resistance of..... 149

HARLEY, ROBERT, on Impossible and certain other Surd Equations... 207
Heat, some Remarks on, and the Constitution of Elastic Fluids......... 107
HOPKINS, THOMAS, on the Formation of Dew ........ 46
——— Cause of Unequal Falls of Rain in Cumberland ........ 196

JOULE, J. P., F.R.S., some Remarks on Heat, and the Constitution of Elastic Fluids ........ 107
JUST, JOHN, Faults in Farming........ 93

KIRKMAN, REV. THOS. PENYNGTON, Mnemonic Aids in the Study of Analysis........ 29
——— On Linear Constructions ........ 279

Lead, Remarks on a Vein of, found in the Carboniferous Strata in Derbyshire ........ 125
——— Memoir on the Oxides and Nitrates of ........ 130
LEIGH, JOHN, M.R.C.S., on the Composition of the Gas produced by the joint Distillation of Tar and Water at a high temperature...... 243
——— on the Chemical Changes attending the Formation of Coal, and their relation to the Philosophy of Gas-making ........ 250
——— on the Analysis of Gaseous Mixtures........ 297
Light, Heat, &c., Researches into the Identity of, by JOHN GOODMAN, M.D. ........ 80
Lightning, and Lightning Conductors........ 56
Linear Constructions........ 279

Meteorite, Description of a, which fell at Allport........ 146
Meteorites, a description of some supposed, found in seams of Coal... 306
Mnemonic Aids in the Study of Analysis........ 29

Rain, Report on the Fall of ........ 1
——— Cause of Unequal Falls of, in Cumberland ........ 196

PAGE.
SMITH, ROBT. ANGUS, Description of a Meteorite which fell at Allport 146
Sternbergiæ, on the Structure and Affinities of the Plants hitherto known as........................................................................ 340
STURGEON, WILLIAM, on Lightning and Lightning Conductors......... 56

Volvox globator, on the........................................................ 321

WILLIAMSON, Professor W. C., on the Volvox globator .................. 321
——— Professor W. C., on the Structure and Affinities of the Plants hitherto known as Sternbergiæ ........................................ 340

END OF VOLUME NINTH.

BRADSHAW AND BLACKLOCK, PRINTERS,
MANCHESTER AND LONDON.

# LIST OF BOOKS

## PRESENTED TO THIS SOCIETY, SINCE APRIL 11TH, 1848.

| Donors. | Titles of Books. |
| --- | --- |
| S. C. Homersham, C.E. | Report on the Supply of Water to Manchester, Salford, and Stockport. By S. C. Homersham, C.E. |
| Professor Young. | Pamphlet, "On an Extension of a Theorem by Euler." By J. R. Young, Professor of Mathematics, Belfast College. |
| J. F. Miller, Esq. | Report on Rain, for 1847. By Mr. J. F. Miller of Whitehaven. |
| John Goodman, M.D., &c. | Successful Case of Cæsarian Operation, and the Complete Recovery. By John Goodman, M.D. |
| The Council of the British Association. | Report of the British Association for the Advancement of Science, for 1847, 1848, 1849. |
| The Council of the American Philosophical Society. | Transactions of the American Philosophical Society, Vol. X. Part 1. |
| " | Proceedings of the American Philosophical Society. Vol. I., Nos. 40—46. |
| From the Editor. | Littell's Living Age. |
| The Council of the Royal Irish Academy. | Transactions of the Royal Irish Academy. Vol. XXI., Part 2. |
| The Registrar-General. | The Eighth and Ninth Annual Reports of the Registrar-General of Births, Deaths, and Marriages in England. |
| W. P. Ormerod, Esq. | On the Sanitary Condition of Oxford. By W. P. Ormerod, Esq. |
| The Council of the Society of Antiquaries, London. | Archæologia. Vol. XXXII. |

| Donors. | Titles of Books. |
|---|---|
| The Council of the British Association. | Lalande and Lacaille's Catalogue of Stars. |
| Ecole des Mines of Paris. | Annales des Mines. 12th Vol. to the 19th Vol. inclusive. |
| The Council of the Literary and Philosophical Society of Liverpool. | Proceedings of the Literary and Philosophical Society of Liverpool, No. 14, Vol. V. |
| Professor Kupffer. | Annuaire Magnetique et Météorologique des Corps des Ingenieurs des Mines. Nos. 1 & 2 for 1848; Nos. 1 & 2 for 1846. |
| " | Resumés des Observations Météorologiques for 1846. By Professor Kupffer of St. Petersburg. |
| The Council of the Leeds Philosoph. & Literary Society. | Annual Report for 1847, 1848, 1849, 1850, of the Leeds Philosophical and Literary Society. |
| Mr. F. W. Dyer. | The Slave Girl: a Tale of the Nineteenth Century. By F. W. Dyer, Esq. |
| | Report of the Directors of the Dublin University Museum. |
| The Council of the Royal Scottish Society of Arts. | Transactions of the Royal Scottish Society of Arts. Vol. III., Parts 2 & 3. |
| Rev. H. H. Jones. | Description of a Machine for Polishing Specula, with Directions for its Use. By H. Lassel, F.R.S. |
| The Council of the Geneva Society for Physics and Natural History. | Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Vol. II., p. 2. |
| Geo. Wareing Ormerod, M.A. | Outlines of the Principal Geological Features of the Salt Field of Cheshire. By G. W. Ormerod, M.A. |
| Wm. Lassel, F.R.S., &c. | Description of a Machine for Polishing Specula. By Wm. Lassel, F.R.S., &c. |

| Donors. | Titles of Books. |
|---|---|
| THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION. | No. 1 of the Smithsonian Contributions to Knowledge—Ancient Monuments of the Mississipi Valley. |
| " | Third Annual Report of the Smithsonian Institution. |
| MR. SAMUEL SALT. | Statistics and Calculations essentially necessary to Persons connected with Railways or Canals, &c. Calculated and Arranged by SAMUEL SALT. |
| " | Facts and Figures, principally relating to Railways and Commerce. By S. SALT. |
| " | Railway and Commercial Information. By S. SALT. |
| J. FLETCHER MELLON, F.R.A.S., &c. | On the Meteorology of the Lake District. By J. FLETCHER MELLON. |
| MR. H. W. FREELAND. | Poems. By H. W. FREELAND. |
| PROF. HODGKINSON. | Report of the Commissioners appointed to inquire into the Application of Iron to Railway Structures. By EATON HODGKINSON, F.R.S., &c. |
| THE CAMBRIDGE PHILOSOPHICAL SOCIETY. | Transactions of the Cambridge Philosophical Society. Vol. VIII., Parts 3, 4, & 5; Vol. IX., Part 1. |
| | Quarterly Journal of the Chemical Society. |
| THE COUNCIL OF THE ROYAL SOCIETY OF EDINBURGH. | Transactions of the Royal Society of Edinburgh, Vol. XVIII.; Vol. XVI., Part 4 and 5; with No. 31 to 41 of Proceedings. |
| MR. RICHARD BUXTON. | Botanical Guide to the Flowering Plants, Ferns, Mosses, and Algæ, found indigenous within sixteen miles of Manchester. By RICHARD BUXTON. |
| MR. T. H. PASLEY. | The Philosophy which shows the Physiology of Mesmerism. By T. H. PASLEY. |
| THE COUNCIL OF THE ROYAL SOCIETY. | Philosophical Transactions of the Royal Society. Parts 1 and 2, 1848; Parts 1 and 2, 1849, with the address of the President; Parts 1 & 2, 1850; Part 1, 1851. |

| Donors. | Titles of Books. |
|---|---|
| E. J. Lowe, F. R. A. S. &c. | A Treatise on Atmospheric Phenomena. By Edmund Joseph Lowe. |
| " | Prognostications of the Weather, and Signs of Atmospheric Changes. By the same Author. |
| " | Samson's Thermometer Stand. |
| The Council of the Philosophical Society of Glasgow. | Proceedings of the Philosophical Society of Glasgow. |
| The Council of the Cuvierian Society of Cork. | Contributions towards a Fauna and Flora of the County of Cork. By the Cuvierian Society of Cork. |
| By the Author, Mr. A. H. Palmer. | Memoirs, Geographical and Commercial, on Liberia, &c. By A. H. Palmer. |
| From Mr. Wakefield. | Remarks on the Smoke Nuisance. By John Wakefield. |
| William Fairbairn, F. R. S. | An account of the Construction of the Britannia and Conway Tubular Bridges. By William Fairbairn, C.E., F.R.S. |
| " | A New System of Architecture. By Wm. Rose Peckitt. |
| " | An Experimental Enquiry into the Strength of Wrought-Iron Plates. By Wm. Fairbairn, F.R.S., &c. |
| " | Two Lectures on the Construction of Boilers, and on Boiler Explosions; also on the Combustion of Smoke. By Wm. Fairbairn, F.R.S. |
| The Physical Society of Berlin. | Die Fortschritte der Physik im Jahre, 1846, dargestellt von der Physikalischen Gesellschaft zu Berlin, 11 Jahrgang. Redigirt von Dr. G. Karsten. |
| Dr. E. du Bois Reymond. | Die Fortschritte der Physik, for 1847. |
| The Council of the Zoological Society. | 31 Nos. and 3 Reports of the Council of the Zoological Society of London. |
| Mr. J. R. Dyer. | Remarks on Education. |

| Donors. | Titles of Books. |
| --- | --- |
| The Historic Society of Lancashire and Cheshie. | Historic Society of Lancashire and Cheshire —1st, 2nd, and 3rd Vols. of Proceedings and Papers. |
| The Council of the British Association. | The British Association's Catalogue of Stars. |
| From the French Academy. | Mémoires de l'Académie des Sciences de l'Institut de France, t. 20, 21, & 22. Also, Mémoires Preséntés par divers Savants à l'Académie, t. 10 & 11. |
| From Samuel Crompton, M.R.C.S. | The 4th, 6th, 7th, and 8th Edition of the Elements of Chemistry, by the late Dr. Wm. Henry, F.R.S., &c., containing annotations in his handwriting. |
| From the Council of the Statistical Society. | A complete Series of the Journals of the Statistical Society of London. 14 Vols. |
| Professor Stokes. | Sixteen Papers on various Scientific Subjects. By Professor Stokes. |
| J. C. Adams, F.R.S. | An Explanation of the observed Irregularity of the Motions of Uranus. By J. C. Adams, F.R.S. |
| E. Schunck, F.R.S. | On Rubian and its products of Decomposition. By Edward Schunck, F.R.S. |
| From Mr. Hedley. | Letter to Dr. Lardner. By Mr. Hedley, Shield Row, Newcastle. |
| W. F. Stevenson, F.R.S. | On Hydrogen Gas and Water. By W. F. Stevenson, F.R.S. |
| John Allan Brown, lately Director of the Observatory. | Report on the Completion of the publication of the Makerstown Observations; also, Makerstown Magnetical and Meteorological Observations for 1845 and 1846. By J. A. Brown, Director. |
| " | Makerstown Magnetical and Meteorological Observations in 1845 and 1846. |
| Beriah Botfield, Esq. | Notes on the Cathedral Libraries of England. By Beriah Botfield. |
| The Royal Observatory of Edinburgh. | Professor Henderson's Astronomical Observations, made at the Royal Observatory of Edinburgh. Reduced and edited by Charles Piazzi Smyth. |

| Donors. | Titles of Books. |
|---|---|
| John James Wild, Esq. | Letter to Lord Brougham. By John James Wild. |
| The Churchwardens of Manchester. | Report of Evidence taken before the House of Commons on the Manchester Rectory Division Bill. |
| Peter Clare, F.R.A.S., Vice-Pres. | An Account of some Thunder Storms and extraordinary Electrical Phenomena at Manchester. By Peter Clare, F.R.A.S. |
| J. F. Miller, F.R.S. | Meteorological Observations in Cumberland. By J. F. Miller, F.R.S. of Whitehaven. |
| Professor Hare. | Memoir on the Explosion of Nitre. By Robert Hare, M.D., Professor of Chemistry in the University of Pennsylvania. |
| Chas. Daubeny, F.R.S. | Introduction to the Atomic Theory. By Charles Daubeny, F.R.S., &c. |
| M. Emmanuel Liais. | Théorie Mathématique des Oscillations du Barometre. Par M. Emmanuel Liais. |
| Royal Cornwall Polytechnic Society. | Royal Cornwall Polytechnic Society's Eighteenth Annual Report. |
| Comte de Wrontchenko, l'Etat Major du Corps des Ingenieurs. | Annales de l'Observatoire Physique Central de Russie. Par A. T. Kupffer. |
| MM. Rayen, Soubeiran, and Bouillard. | "Digitaline" (2 copies). |
| J. Hastings, Esq., of the Liverpool Observatory. | Meteorological Results, &c., during 1850. By John Hastings, Esq. |
| Rev. Henry Halford Jones. | Philosophy of Education. By H. H. Jones, F.R.A.S. |
| The Society of Civil Engineers. | Proceedings and Transactions of the Society of Civil Engineers, from the Commencement. |
| Wm. Johnson, Esq. | Climate of Sidmouth, by W. H. Cullen, M.D. |

# THE COUNCIL

OF

# The Literary and Philosophical Society

OF MANCHESTER.

APRIL 16TH, 1851.

---

**President:**

PROF. EATON HODGKINSON, F.R.S., M.R.I.A., F.G.S., &c.

**Vice-Presidents:**

JOHN MOORE, F.L.S.
PETER CLARE, F.R.A.S.
JOSEPH ATKINSON RANSOME, F.R.C.S.
WILLIAM FAIRBAIRN, F.R.S., M. INST. C.E.

**Secretaries:**

JAMES P. JOULE, F.R.S., &c.
EDWARD WILLIAM BINNEY.

**Treasurer:**

SIR BENJAMIN HEYWOOD, BART., F.R.S.

**Librarian:**

E. W. MAKINSON, A.M.

**Of the Council:**

THOMAS HOPKINS
REV. H. H. JONES, F.R.A.S.
JOHN GRAHAM.
WILLIAM FLEMING, M.D.
LAURENCE BUCHAN.
ROBERT ANGUS SMITH, PH. D.

AN

# ALPHABETICAL LIST

OF THE

# MEMBERS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

## OF MANCHESTER.

APRIL 16TH, 1851.

---

| | DATE OF ELECTION. |
|---|---|
| James Ainsworth | January 25th, 1805 |
| Ralph F. Ainsworth, M.D | April 30th, 1839 |
| Thomas Ashton, Mosley Street | October 29th, 1824 |
| Thomas Ashton, Hyde | August 11th, 1837 |
| John Atkinson | January 27th, 1846 |
| W. H. Ash | April 17th, 1849 |
| | |
| Richard Parr Bamber, F.L.S. | October 19th, 1821 |
| Robert Barbour | January 23rd, 1824 |
| Joseph Barratt | April 19th, 1842 |
| John Frederick Bateman, M. Inst. C.E. | January 21st, 1840 |
| Thomas Bazley | January 26th, 1847 |
| Charles Bell, M.D | January 25th, 1848 |
| William Bell | January 26th, 1847 |
| James Bevan | January 23rd, 1844 |
| Edward William Binney | January 25th, 1842 |
| Alfred Binyon | January 26th, 1838 |
| Richard Birley | April 18th, 1834 |
| James Black, M.D., F.G.S. | April 30th, 1839 |

| | DATE OF ELECTION. |
|---|---|
| John Blackwall, F.L.S. | January 26th, 1821 |
| Henry Bowman | October 29th, 1839 |
| Edward Brooke | April 30th, 1824 |
| W. C. Brooks, M.A. | January 23rd, 1844 |
| Henry Browne, M.B. | January 27th, 1846 |
| Laurence Buchan | November 1st, 1810 |
| John Burd | January 27th, 1846 |
| Rev. R. Bassnett, A.M. | April 17th, 1849 |
| | |
| Henry Cadogan Campbell | January 23rd, 1835 |
| Frederick Crace Calvert, M.R.A.T. | January 26th, 1847 |
| John Young Caw | April 15th, 1841 |
| Henry Charlewood | January 24th, 1832 |
| David Christie | October 19th, 1847 |
| Peter Clare, F.R.A.S. | April 27th, 1810 |
| Charles Clay, M.D. | April 15th, 1841 |
| Rev. John Colston | October 29th, 1850 |
| Thomas Cooke, jun. | April 12th, 1838 |
| Samuel Elsworth Cottam, F.R.A.S. | October 20th, 1837 |
| James Crossley | January 22nd, 1839 |
| Joseph S. Crowther | January 25th, 1848 |
| Charles Cumber | November 1st, 1833 |
| Matthew Curtis | April 18th, 1843 |
| | |
| John Benjamin Dancer | April 19th, 1842 |
| Samuel Dukinfield Darbishire | January 25th, 1822 |
| Rev. John Davies, M.A. | January 21st, 1851 |
| James Joseph Dean | November 15th, 1842 |
| Joseph Cheeseborough Dyer | April 24th, 1818 |
| Frederick Nathaniel Dyer | April 30th, 1850 |
| | |
| The Right Hon. the Earl of Ellesmere, F.G.S. | April 15th, 1841 |
| | |
| Thomas Fairbairn | April 30th, 1850 |
| William Fairbairn, F.R.S., M. Inst. C.E. | October 29th, 1824 |
| W. A. Fairbairn | October 30th, 1849 |
| Octavius Allen Ferris | January 26th, 1847 |
| David Gibson Fleming | January 25th, 1842 |
| William Fleming, M.D. | April 18th, 1828 |

DATE OF ELECTION.

Richard Flint....................October 31st, 1818
James William Fraser....................January 22nd, 1839

Rev. William Gaskell, M.A....................January 21st, 1840
Samuel Giles....................April 20th, 1836
Thomas Glover....................January 21st, 1831
John Goodman, M.D....................January 25th, 1842
John Gould....................April 20th, 1847
John Graham....................August 11th, 1837
Robert Hyde Greg, F.G.S. ....................January 24th, 1817
William Rathbone Greg....................April 26th, 1833
Robert Philips Greg....................October 30th, 1849
John Edgar Gregan....................January 25th, 1848
John Clowes Grundy....................January 25th, 1848

Rev. Robert Halley, D.D....................April 29th, 1845
Richard Hampson....................January 23rd, 1844
John Hawkshaw, F.G.S. and M. Inst. C.E. ....................January 22nd, 1839
William Charles Henry, M.D., F.R.S....................October 31st, 1828
Sir Benjamin Heywood, Bart., F.R.S. ....................January 27th, 1815
James Heywood, M.P., F.R.S. and G.S. ....................April 26th, 1833
James Higgins....................April 29th, 1845
Peter Higson....................October 31st, 1848
John Hobson.................... January 22nd, 1839
Eaton Hodgkinson, F.R.S., M.R.I.A., F.G.S., &c. January 21st, 1820
James Platt Holden....................January 27th, 1846
Thomas Hopkins....................January 18th, 1823
Henry Houldsworth....................January 23rd, 1824

Paul Moon James....................January 27th, 1837
John Jesse, F.R.S., R.A.S., and L.S. ....................January 24th, 1823
Rev. Henry Halford Jones, F.R.A.S. ....................April 21st, 1846
Joseph Jordan....................October 19th, 1821
James Prescott Joule, F.R.S., &c....................January 25th, 1842
Benjamin Joule, jun. .................... April 18th, 1848
William Joynson....................January 27th, 1846
Richard Johnson....................April 30th, 1850

| | DATE OF ELECTION. |
|---|---|
| Alexander Kay | October 30th, 1818 |
| Samuel Kay | January 24th, 1843 |
| John Kennedy | April 29th, 1803 |
| | |
| Richard Lane | April 26th, 1822 |
| William Langton | April 30th, 1830 |
| John Leigh | April 17th, 1849 |
| John Rowson Lingard | January 26th, 1847 |
| Thomas Littler | January 27th, 1825 |
| John Lockett | January 25th, 1842 |
| Joseph Lockett | October 29th, 1839 |
| Benjamin Love | April 19th, 1842 |
| Joseph Leese, jun. | April 30th, 1850 |
| Edward Lund | April 30th, 1850 |
| | |
| James M'Connel | October 30th, 1829 |
| William M'Connel | April 17th, 1838 |
| Alexander Macdougal | April 30th, 1844 |
| John Macfarlane | January 24th, 1823 |
| Edward William Makinson, B.A. | October 20th, 1846 |
| The Right Rev. the Lord Bishop of Manchester, D.D., F.R.S., F.G.S. | April 17th, 1849 |
| Robert Manners Mann.. | January 27th, 1846 |
| James Meadows | April 30th, 1830 |
| Thomas Mellor | January 25th, 1842 |
| William Mellor | January 27th, 1837 |
| John Moore, F.L.S. | January 27th, 1815 |
| L. A. J. Mordacque | October 29th, 1830 |
| David Morris .. | January 23rd, 1849 |
| George Murray | January 27th, 1815 |
| | |
| Alfred Neild | January 25th, 1848 |
| William Neild | April 26th, 1822 |
| John Ashton Nicholls, F.R.A.S. | January 21st, 1845 |
| William Nicholson | January 26th, 1827 |
| | |
| George Wareing Ormerod, M.A., F.G.S. | January 26th, 1841 |
| Henry Mere Ormerod | April 30th, 1844 |

| | DATE OF ELECTION. |
|---|---|
| John Owen | April 30th, 1839 |
| Joseph Owen | February 5th, 1850 |
| | |
| George Parr | April 30th, 1844 |
| John Parry | April 26th, 1833 |
| George Clark Pauling | January 25th, 1842 |
| George Peel, M. Inst. C.E. | April 15th, 1841 |
| Peter Pincoffs, M.D. | January 25th, 1848 |
| Archibald Prentice | January 22nd, 1819 |
| | |
| Joseph Atkinson Ransome, F.R.C.S | April 29th, 1836 |
| Thomas Ransome | January 26th, 1847 |
| Rev. William Read, A.M. | January 23rd, 1824 |
| Rev. John Gooch Robberds | April 26th, 1811 |
| Richard Roberts, M. Inst. C.E. | January 18th, 1823 |
| Samuel Robinson | January 25th, 1822 |
| Alan Royle | January 25th, 1842 |
| Samuel Salt | April 18th, 1848 |
| Michael Satterthwaite, M.D. | January 26th, 1847 |
| Edward Schunck, Ph. D., F.R.S. | January 25th, 1842 |
| Salis Schwabe | April 20th, 1847 |
| John Sharp | October 28th, 1824 |
| John Shuttleworth | October 30th, 1835 |
| George S. Fereday Smith, M.A., F.G.S. | January 26th, 1838 |
| Robert Angus Smith, Ph. D. | April 29th, 1845 |
| Edward Stephens, M.D | January 24th, 1834 |
| James Stephens | April 20th, 1847 |
| Daniel Stone, jun. | January 23rd, 1849 |
| Robert Stuart | January 21st, 1814 |
| | |
| Rev. John James Tayler, B.A. | January 26th, 1821 |
| John Thom | January 27th, 1846 |
| James Aspinal Turner | April 29th, 1836 |
| Thomas Turner, F.R.C.S | April 19th, 1821 |
| | |
| Absolom Watkin | January 24th, 1823 |
| Joseph Whitworth | January 22nd, 1832 |
| Matthew A. Eason Wilkinson, M.D. | January 26th, 1841 |
| William James Wilson, F.R.C.S. | April 29th, 1814 |

| | DATE OF ELECTION. |
|---|---|
| Gilbert Winter | November 2nd, 1810 |
| George Bancroft Withington | January 21st, 1851 |
| William Rayner Wood | January 22nd, 1839 |
| George Woodhead | April 21st, 1846 |
| Edward Woods | April 30th, 1839 |
| Robert Worthington | April 28th, 1840 |
| James Woolley | November 15th, 1842 |
| | |
| Joseph St. John Yates | January 26th, 1841 |
| James Young | October 19th, 1847 |